THURSDAY, APRIL 10, 1884

STOKES ON LIGHT

Burnett Lectures. On Light. First Course, On the Nature of Light. By G. G. Stokes. (London: Macmillan, 1884.)

BYRON once wrote about a work of art:—

> "What nature could, but would not, do,
> And beauty and Canova can."

A dozen, or more, years ago, the scientific world was excited by the announcement that a treatise on *Light* was to be published by Prof. Stokes, as a companion volume to Clerk-Maxwell's remarkable *Theory of Heat*. The announcement was, however, ultimately withdrawn. Nature could, but would not, do it.

But Beauty and Canova, in the form of a Conservative Government and its *Endowed Institutions (Scotland) Act* of six years ago, did it:—and we now have, as a result, the first of a series of three volumes by Prof. Stokes!

The Burnett foundation, now about a century old, was essentially of a Teleological character. It had been applied, in accordance with the Founder's will, at intervals of 40 years, to the award of prizes for Essays:—the competition being perfectly open. Those who know what Teleology was, half-a-century ago or more:—even at its very best, as in the once-celebrated *Bridgewater Treatises*:—will probably be of opinion that the Commissioners, under the Act referred to, did real good by their modification of the terms of the Burnett Endowment.

True:—it is still possible that the Trustees may some day appoint to the Burnett Lectureship a rabid Teleologist of the old school. Even this might be defended on the ground of even-handed justice; though an ill-informed, but zealous, champion of such a cause is probably more dangerous to it than is a declared enemy. But the appointment of Prof. Stokes, as the first holder of the new office, augurs well for the future. Beside his scientific qualifications, he is possessed of that calm, judicial, mind which is absolutely required in so delicate a position:—where, indeed, to say too little might involve the charge of luke-warmness, if not of positive unbelief; while to say too much would display that presumption which is usually characteristic of ignorance.

As the title of the work implies, the subject is the nature and extent of the present evidence in favour of the Undulatory Theory of Light. The reader is supposed to have an elementary acquaintance with the simpler facts of geometrical optics, nothing further. Hence the work is, in the true sense, a popular one; suited to any reader of average intelligence and information. The language is simple and, as far as possible, devoid of technicalities:—while it "goes without saying" that the author does not condescendingly patronise the assumed weakness of his reader, nor does he anywhere attempt to escape from a difficulty by the use of mysterious or indefinite expressions.

As the book will, undoubtedly, find its way into the hands of every one who desires to see an important and difficult subject brought by a Master to the level of the understanding of any ordinary reader, we will content ourselves with a few selections. These have been made on a very catholic principle, some to exhibit novelties of matter or treatment, some for their historical bearing, and one to show how the special difficulty of the Lecturer's position has, so far, been met.

First, we have the two rival theories of Light compared, with the chief arguments for and against each:—

"*Primâ facie* there is much to be said in favour of the theory of emission. It lends itself at once to the explanation of the rectilinear propagation of light, and the existence of rays and shadows. It falls in at once with the law of aberration. The laws of reflection and refraction admit of an easy explanation in accordance with it; at least if we accept the existence of both reflection and refraction; for according to this theory we should rather have expected beforehand that light would have been *either* reflected or refracted, according to circumstances, not that incident light should have been *divided* into a portion reflected and a portion refracted.

"The theory of undulations on the other hand presents at the outset considerable difficulties. In the first place it requires us to suppose that the interplanetary and interstellar spaces are not, strictly speaking, a vacuum but a plenum; that though destitute of *ponderable* matter they are filled with a substance of some kind, constituting what we call a *medium*, or vehicle of transmission of the supposed undulations. When I speak of this medium as a substance, or as material, I mean that it must possess that distinctive property of matter, inertia; that is to say, a finite time must be required to generate in a finite portion of it a finite velocity."

Then the special difficulty which made Newton abandon the wave-theory:—

"The necessity of assuming the existence of some kind of substance in what we commonly speak of as a vacuum, does not appear to have been a serious preliminary difficulty in the way of the reception of the theory of undulations. A far more formidable difficulty appeared at first to be presented by the existence of rays and shadows. It was this that led Newton to adopt the theory of emission, though even he was led in the course of his researches on light to suppose that there was some sort of medium through which the particles of light moved, and in which they were capable in certain cases of exciting a sort of undulation. But the supposition of particles darted forth seemed to him necessary to account for shadows."

How, mainly by the marvellous insight of Young, this difficulty has been, not merely got over but, converted into one of the strongest arguments in favour of the Undulatory Theory:—

"There is no difference of explanation as regards light and as regards sound, save what depends on the difference of scale entailed by the difference of wave-length. Take as regards light the case of a small circular hole, say the tenth of an inch in diameter, and of distances from the luminous point to the screen in which the hole is pierced, and from that again to the screen on which the light is received, of say 8 feet 4 inches, or 100 inches, each. In this case, regarding the luminous patch on the screen as a whole, there would be no great diffusion of light, but the phenomena of diffraction would nevertheless be fairly pronounced. There ought to be a corresponding case of diffraction for sound; but on what scale? Take 50 inches as the length of a wave of sound, which would correspond to a musical note of moderate pitch. Taking as before the 1/50000 part of an inch as the wave-length for light, the length of the wave of sound will be two-and-a-half million times as great as the wave-length of light. Consequently to obtain the corresponding case of diffraction for sound, our 'small' circular hole

would be obliged to have a diameter of rather more than four miles, say four miles, and the distances from the source of sound to the hole through which it passes, and from that again to the place where the sound is listened to, would have to be 4000 miles each.

"It is remarkable that the existence of rays, which formed the great stumbling-block in the way of the early reception of the theory of undulations, is now shown to belong to a class of phenomena, those of diffraction, the complete and marvellously simple explanation of which afforded by the theory of undulations now forms one of the great strongholds of that theory."

In connection with the Lecture on the *Senses*, by Sir W. Thomson, which has recently appeared in NATURE (vol. xxix. pp. 438, 462) we may take the following passage. [At the same time it may be well to remark, in passing, that Sir W. Thomson omits altogether the Sense of *Rotation*, which seems to be fully established by the researches of Crum Brown, De Cyon, Flourens, Mach, &c. He also distinguishes between the Senses of Touch and of Heat, making the so-called Muscular Sense a case of the former; while it seems more probable that Touch and Heat are the same sense, and the Muscular sense an independent one.]

"As regards the mode of perception, while there are analogies between sound and light there are at the same time notable differences. In sound, the tympanum of the ear is thrown mechanically into vibration, and the nerves of hearing are mechanically affected, as a mechanical disturbance of a point on the surface of the body is made known by the sense of touch. But in light, just as we have seen reason to believe that it is the disturbance of the ultimate molecules, or of their constituent parts, by which the vibratory motion which constitutes light is in the first instance communicated from ponderable matter to the ether, so we have reason to think that when light is absorbed what takes place is that the disturbance of the ether is communicated, not to portions of matter regarded as forming portions of a continuous elastic body, but to the ultimate molecules of which matter consists, or to their constituent parts. It may be that temporary chemical changes are thereby produced in the ultimate filaments of the nerves of the retina, in which case the sense of sight would be more analogous to the sense of taste than to that of touch."

As a specimen of the firm, yet cautious, way in which the Lecturer meets the grand difficulty of his position, take the following :—

"In studying this subject, one can hardly fail to be struck with the combination of these two things :—the importance of the ends, the simplicity of the means. When I say the importance of the ends, I use a form of expression which is commonly employed as expressing design. And yet on that very account we must be on our guard against too narrow a view. When we consider the subject of vision in its entirety, the construction of the recipient organ as well as the properties of the external agent which affects it, the evidence of design is such, it seems to me, as must to most minds be irresistible. Yet if I may judge of other men's minds by my own, it is rather in the construction of the recipient organ than in the properties of the agent that affects it that the evidence of design is so strongly perceived. And the reason of this may be that we are here dealing with what more nearly resembles design as we know it in ourselves. Man takes the laws of matter as he finds them; the laws of cohesion, of the conversion of liquid into vapour, of the elasticity of gases and vapours, and so forth; and in subserviency to those laws he constructs a machine, a steam-engine for instance, or whatever it may be; but over the laws themselves he has absolutely no control. Now when we contemplate the structure of the eye we think of it as an organ performing its functions in subserviency to laws definitely laid down, relating to the agent that acts upon it, laws which are not to be interfered with. We can, it is true, go but a little way towards explaining how it is that through the intervention of the eye the external agent acts upon the mind. Still, there are *some* steps of the process which we *are* able to follow, and these are sufficient to impress us strongly with the idea of design. The eye is a highly specialised organ, admirably adapted for the important function which it fulfils, but, so far as we can see, of no other use; and this very specialisation tends to make the evidence of design simpler and more apparent. But when we come to the properties of the external agent which affects the eye, we begin to get out of our depth. These more nearly resemble those ultimate laws of matter over which man has no control; and to say that they were designed for certain important objects which we perceive to be accomplished in subserviency to them, seems to savour of presumption. It is but a limited insight that we can get into the system of nature; and to take the very case of the luminiferous ether, while as its name implies it is all-important as regards vision, the present state of science enables us to say that it serves for one object of still more vital importance; we seem to touch upon another; and there may be others again of which we have no idea."

At the end of the work we are told that the two volumes, which are to follow this, are to deal with

II. *Researches in which Light has been used as a means of investigation*, and

III. *Light, considered in relation to its beneficial effects.*

The former of these we may hope to have in a year from the present time; for the final volume we must wait a year longer. But in the meantime let us be thankful for the first instalment, which is a masterpiece of simplicity and strength; and be grateful to the Commission, and the Trustees, to whom we are so very directly indebted for it. And, above all, let us lay to heart the valuable lesson which the Author has drawn from the story of the two rival theories of Light, and of their chief supporters, a lesson good for all time :—

"It may be said, If the former theory is nowadays exploded, why dwell on it at all? Yet surely the subject is of more than purely historical interest. It teaches lessons for our future guidance in the pursuit of truth. It shows that we are not to expect to evolve the system of nature out of the depths of our inner consciousness, but to follow the painstaking inductive method of studying the phenomena presented to us, and be content gradually to learn new laws and properties of natural objects. It shows that we are not to be disheartened by some preliminary difficulties from giving a patient hearing to a hypothesis of fair promise, assuming of course that those difficulties are not of the nature of contradictions between the results of observation or experiment and conclusions certainly deducible from the hypothesis on trial. It shows that we are not to attach undue importance to great names, but to investigate in an unbiased manner the facts which lie open to our examination."

On this it would be impertinent to make any farther comment.

P. G. TAIT

OUR BOOK SHELF

Absolute Measurements in Electricity and Magnetism. By Andrew Gray, M.A., F.R.S.E. (London : Macmillan and Co., 1884).

THIS book, which is mainly a reprint of a series of papers on absolute measurement of electric currents and poten-

tials which appeared in these columns a short time ago, but with some additional matter, must, from the clear explanation of the principles involved in the different methods of measurement, take a high position as an educational work, and, from the care with which details of manipulation are in many parts described, form a valuable laboratory guide.

The author begins by explaining Gauss's method of finding the horizontal intensity of the earth's magnetism. Instead of describing an "instrument-maker's" magnetometer, and showing how with this expensive luxury H may be determined, he gives simple, clear, and full directions for constructing, with such common materials as are to be found in any laboratory, all that is necessary for making this determination with great accuracy.

A description of the tangent galvanometer in some of its forms and an explanation of some of the units naturally follow. Here, by treating each unit separately with many illustrations depending on the aspect from which they are viewed, the author has succeeded in giving them a reality which students often find it difficult to believe they possess.

The next two chapters are devoted to a description of the construction and graduation of Sir W. Thomson's "Graded Galvanometers." These instruments possess so great a range, and are, when used carefully in the laboratory, so accurate and convenient, though rather delicate for an engine-room, that an exact description from headquarters of their construction, of the precautions which must be observed in their use, and of the means of graduating them is especially valuable.

The various methods employed in measuring any resistance from that of a thick copper rod to that of a piece of gutta-percha are given, and in many cases explained by numerical examples.

The methods by which the energy due to direct or to alternating currents may be measured is explained—in the latter case on the assumption that the current strength varies harmonically with the time.

The chapter on the measurement of intense magnetic fields is especially interesting, for the methods given, depending on the use of suspended bits of wire attached by threads to pendulum weights, or equally simple and easily contrived devices, show how the experimenter may in many cases be independent of the elaborate work of the instrument-maker. C. V. B.

Field and Garden Crops of the North-Western Provinces and Oudh. By J. F. Duthie, B.A. F.L.S., Superintendent of the Saharanpur Botanical Gardens, and J. B. Fuller, Director of Agriculture, Central Provinces. Part 2. With Illustrations.

As a work of reference it will be very valuable, for it contains well-arranged details of some of the more important crops under cultivation, and the information is well and systematically arranged. Care has been taken in each case to secure a complete but still a concise statement, which is sufficient to guide the cultivator in all the specialities of management necessary to secure successful results. A good drawing illustrates each crop treated of, and its several cultivated varieties, and with these we have carefully-prepared descriptions of each plant in succession, and its general history. The districts within which the cultivation can be successfully extended are also set forth with great clearness and precision. For accuracy of details, in a very accessible form, this work leaves little to be desired.

A Treatise on Higher Trigonometry. By the Rev. J. B. Lock. (Macmillan, 1884.)

THIS is the promised complement to the same writer's "Treatise on Elementary Trigonometry," which we noticed very favourably in these pages at the time of its appearance (vol. xxvi. p. 124). It is concerned principally with series, the errors which arise in practical work, and the use of subsidiary angles in numerical calculations.

A short chapter on the use of imaginaries is justified by the position this subject holds in the London University Examinations, and no apology is needed for the space assigned to an account of, and a collection of exercises upon, the hyperbolic sine and cosine. We have read the text carefully, and though almost of necessity there are numerous typographical mistakes, only one or two (for $2a \cos 2\theta$, p. 127, line 3, read $a \cos 2\theta$) will inconvenience a student. In addition to the numerous examples in the text, there are fourteen specimen papers from Cambridge and other examinations.

The only article to which we take exception is § 9, the proof of which may be, if we mistake not, considerably simplified. The book can be confidently recommended to the use of advanced pupils in our schools, and will meet the wants of most students in our Universities.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Teaching Animals to Converse

YOU did me the honour some weeks ago (January 3, p. 216) to insert a letter of mine, containing suggestions as to a method of studying the psychology of animals, and a short account of a beginning I had myself made in that direction.

This letter has elicited various replies and suggestions which you will perhaps allow me to answer, and I may also take the opportunity of stating the progress which my dog "Van" has made, although, owing greatly no doubt to my frequent absences from home, and the little time I can devote to him, this has not been so rapid as I doubt not would otherwise have been the case. Perhaps I may just repeat that the essence of my idea was to have various words, such as "food," "bone," "water," "out," &c., printed on pieces of cardboard, and after some preliminary training, to give the dog anything for which he asked by bringing a card.

I use pieces of cardboard about 10 inches long and 3 inches high, placing a number of them on the floor side by side, so that the dog has several cards to select from, each bearing a different word.

One correspondent has suggested that it would be better to use variously coloured cards. This might no doubt render the first steps rather more easy, but, on the other hand, any temporary advantage gained would be at the expense of subsequent difficulty, since the pupil would very likely begin by associating the object with the colour rather than with the letters; he would, therefore, as is too often the case with our own children, have the unnecessary labour of unlearning some of his first lessons. At the same time the experiment would have an interest as a test of the condition of the colour-sense in dogs. Another suggestion has been that, instead of words, pictorial representations should be placed on the cards. This, however, could only be done with material objects, such as "food," "bone," "water," &c., and would not be applicable to such words as "out," "pet me," &c.; nor even as regards the former class do I see that it would present any substantial advantage.

Again, it has been suggested that "Van" is led by scent rather than by sight. He has no doubt an excellent nose, but in this case he is certainly guided by the eye. The cards are all handled by us, and must emit very nearly the same odour. I do not, however, rely on this, but have in use a number of cards bearing the same word. When, for instance, he has brought a card with "food" on it, we do not put down the same identical card, but another with the same word; when he has brought that, a third is put down, and so on. For a single meal, therefore, eight or ten cards will have been used, and it seems clear, therefore, that in selecting them "Van" must be guided by the letters.

When I last wrote I had satisfied myself that he had learnt to regard the bringing of a card as a request, and that he could distinguish a card with the word "food" on it from a plain one, while I believed that he could distinguish between a card with "food" on it, and one with "out" on it. I have no doubt that he can distinguish between different words. For instance, when he is hungry he will bring a "food" card time after time until he has had enough, and then he lies down quietly for a nap. Again, when I am going for a walk and invite him to come, he gladly responds by picking up the "out" card and running triumphantly with it before me to the front door. In the same way he knows the "bone" card quite well. As regards water (which I spell phonetically so as not to confuse him unnecessarily), I keep a card always on the floor in my dressing-room, and whenever he is thirsty he goes off there, without any suggestion from me, and brings the card with perfect gravity. At the same time he is fond of a game, and if he is playful or excited will occasionally run about with any card. If through inadvertence he brings a card for something he does not want, when the corresponding object is shown him he seizes the card, takes it back again, and fetches the right one.

No one who has seen him look along a row of cards and select the right one can, I think, doubt that in bringing a card he feels that he is making a request, and that he can not only perfectly distinguish between one word and another, but also associate the word and the object.

I do not for a moment say that "Van" thus shows more intelligence than has been recorded in the case of other dogs; that is not my point, but it does seem to me that this method of instruction opens out a means by which dogs and other animals may be enabled to communicate with us more satisfactorily than hitherto.

I am still continuing my observations, and am now considering the best mode of testing him in very simple arithmetic, but I wish I could induce others to cooperate, for I feel satisfied that the system would well repay more time and attention than I am myself able to give. JOHN LUBBOCK

High Elms, Down, Kent

"The Unity of Nature"

I REGRET that the Duke of Argyll should have been led by anything that I have written to make some of the remarks which appear in this week's issue of NATURE (p. 524). If a reviewer in a signed review cannot express freely his opinion upon a book without its being suggested that he is actuated by secondary and sinister motives, I fancy that few men of common honesty would care to continue the work of reviewing. Moreover, in the present instance the imputation of animus seems to me specially unjustifiable. I had almost forgotten the correspondence in NATURE to which the Duke alludes, but on now referring to it again I can only see that, if it was provocative of animus, there was assuredly no reason for the animus to have arisen on my side (see NATURE, vol. xxiv. pp. 581 and 604; vol. xxv. pp. 6 and 29). But, to ignore so unworthy a charge, and one which I can only suppose to have been made under a sense of irritation, I must explain that the Duke is under a wrong impression when he assumes that my objection to his advocacy of Theistic belief is due to what he regards as my aversion to Theism. As I have never been in the habit of "using your columns for the purpose of inculcating personal beliefs and disbeliefs on subjects which lie outside the boundaries of physical science," I shall not do so now. But in view of the slender grounds on which the Duke has felt himself entitled to infer that I "hold that the highest aim of the human intellect is to prove the mindlessness of nature," I feel it is desirable to correct the inference. For this purpose it is not needful that I should publish my "personal beliefs and disbeliefs." It is only needful to say that my previous remarks will be found to have been directed, not against the cause of Theism, but against its champion in the Duke of Argyll. Had my sympathies been more on the side of the materialists than they happen to be, the Duke of Argyll might not have found so much reason to quarrel with my "dislike" of his advocacy.

I may now turn to the Duke's remarks on those of my criticisms which he deems legitimate. Taking first the case of rudimentary organs, I quite agree with the statement that the question whether any particular structure now dissociated from use is to be regarded as "on the stocks or on the wane" is "a question of evidence from associated facts." Therefore it was that I said in my review that no illustration could be more unfortunate than the one which was chosen by the Duke as an example of rudimentary structures possibly on the stocks. For if the rudimentary organs which occur in the Cetacea admit of being supposed of doubtful interpretation in this matter, it is clear that in no case could the "evidence from associated facts" of structure and affinity be of any value. But in reality this evidence is nearly always so cogent that the difficulty suggested by the Duke is of a purely imaginary kind: evolutionists have no need ever to be puzzled in deciding whether a given structure is on the stocks or on the wane. Thus, for instance, let us take the cases which are adduced by the Duke himself. No evolutionist could be insane enough to imagine that the papillæ on the roof of the mouth of the giraffe are the remnants of whalebone, seeing that the whole structure and all the affinities of the animal are opposed to the inference that its ancestors were aquatic mammalia. Or, if we take the case of webbed feet, even if the dipper had begun to develop them, no evolutionist in his senses would infer that these incipient structures were remnants of structures once more fully developed, seeing that all the other structures and affinities of the bird prove that it belongs to a non-aquatic family. Cases of this kind actually occur in such birds as the grebe and the coot, where even apart from structure and affinity it is easy to see that the little piece of web must be regarded as a growing and not a dwindling organ, seeing that the birds are so strongly aquatic in their habits.

Considering next the Duke's remarks on instinct, I did not attempt to deal with the argument to which he refers, because I could not perceive that there was any argument to be dealt with. His view is a mere assumption to the effect that instincts are divinely implanted intuitions independent of experience; and to deny that experience, *in successive generations*, is the source of instinct is not to meet, by way of argument, the enormous mass of evidence which goes to prove that such is the case. Even within the limits of my review I should have thought there was evidence enough to have disposed of this denial.

As for the special case of the dipper, I only mentioned it in my review because the Duke lays great stress upon it in his book. No doubt better cases occur of newly-acquired instincts not yet associated with correlated structures, and in all such cases (whether good, bad, or indifferent), it is not a *non sequitur* mode of argument to say that, on the theory of the transmutation of instincts, the appropriate organs have not been developed, because, looking to the affinities of the animal, we are entitled to infer that time enough has not yet been allowed for their development. Again, I deny that it is for me, or for any other evolutionist, to prove that the ancestors of the dipper did not present those lesser modifications of structure which, according to the Duke, are now correlated with the aquatic instincts.[1] By "proof" he no doubt means the display of the ancestral form, and not the study of allied species. Proof of this kind is not attainable, but neither is it required. The question whether instincts are fixed intuitions or admit of being modified by accumulative experience with natural selection—*i.e.* whether they are or are not subject to evolution—is a question that does not require to be settled on the narrow basis of any one particular case. And if we take a broad view of all the instincts known to us, the combined weight of their testimony to the fact of transmutation is simply overwhelming.

London, April 4 GEORGE J. ROMANES

[1] I say "according to the Duke," because, according to Mr. Darwin, "in the case of the water-ouzel the acutest observer, by examining its dead body, would never have suspected its sub-aquatic habits" ("Origin of Species," 6th ed., p. 142).

The Remarkable Sunsets

THE remarkable *red sunsets* and after-glows, about which so much has been written of late, still continue here, but in a less intense form. A remarkable one occurred last night, and while watching it I determined to send you a brief account of my experiences in the matter. It is of little use going into descriptions of the appearances which are now well known, but the one which occurred last evening was unusually fine. It was a stormy wild evening, with black clouds all around, except in the west, where, from about 10° above the horizon to near the zenith, it was quite clear, and of a pale orange glow. A quarter of an hour after sunset three immense rays through rifts in the cloud bank sprang up almost suddenly, and took quite an intense crimson lake colour, which lasted about ten minutes.

Our brightest displays occurred in October and November last, and frequently bathed the whole landscape in a deep

crimson glow. These skies were often still more gorgeous in the morning, and on some occasions were so wonderful as to be styled *frightful* by some observers. I witnessed one of these sunrises from an altitude of 3000 feet in January, and it was almost an *awful* sight. The view to the east was over about thirty miles of plains to distant mountains; a low mist hung over the low ground, and the surface appeared slightly rolling as seen from above. The sky half an hour before sunrise was so intensely red, almost to the zenith, that it gave this mist the appearance of a sea of blood. Every object, tree-trunks, fern-trees, bushes, rocks, and the cottages about the hills, was of a similar lurid colour; still there was not yet sufficient light to read by comfortably. This display reminded me of the wonderfully red aurora witnessed in Australia on April 5, 1870, when the red light was so intense that ordinary newspaper type could be read by it at ten o'clock on a moonless night, the type appearing as if set in a blood-red sheet. This was the first time I recorded the *red spectrum line* of the aurora, and I think was one of the earliest observations of this fact.

Some of the recent sunsets have looked very much like an aurora in the west, and faint traces of stratification lent additional similarity; indeed on one night early in December, the *after-glow* merged into a beautiful aurora, and silver streamers were seen before all the red glow had disappeared.

From all over Australia reports of wonderful sunsets and sunrises have been sent to me. In one case the *red glow* was reported as margined by an immense *black bow* stretching across from north-west to south-west. On several occasions these glows prolonged the twilight considerably, and a correspondent at Urana, in New South Wales, described one occasion where approaching darkness after one of these sunsets at length compelled him to leave off watering his garden, but suddenly the light increased again sufficiently to induce him to resume his work; and he states that a similar accession of light—each time fainter—occurred on that same evening.

The season over the south of Australia especially, but all over the continent, has been remarkable, and, so far as this colony is concerned, unprecedented in my thirty-three years' knowledge of the climate. January, February, and March are usually our dry, hot months; this year they have been wet and cold ones. The average rainfall for January has been 1·60 inches; this year it was 4·75 inches. For February the average is 1·95 inches, and up to this date (the 27th) it has been also 1·95 inches. The mean temperature for January was 3°·5 below the average, and for February 2° below. Stormy, squally, wintry weather has predominated, with now and then a very hot or a tropical day for a change.

Even before the Krakatoa outburst the northern parts of Tasmania had become subject to prolonged *earth tremors*, with now and then a decided earthquake shock. These disturbances still continue, and appear to be extending northwards, for on the 15th of this month a shock was felt at Gabo Island, at the south-east extremity of Australia, and a very severe one again on the 17th, when a curious and sudden barometric disturbance, not unlike that at the time of the Java catastrophe, was shown on our barographs.

While on this subject it may be as well to state that Mr. Barrachi, one of my assistants, while at Port Darwin determining the difference of longitude between that place and Singapore in March 1883, saw sunsets, followed by after-glows, which prolonged the usual short twilights to a very considerable extent, and he states they were equally remarkable with those witnessed here. They only occurred either just before or just after very heavy rains.

Referring to the various hypotheses which have found their way into print explanatory of the unusual phenomena attending sunrise and sunset since August 1883, the belief that they have been in some way brought about by the Krakatoa eruption appears to be generally accepted, and while some doubt may be thrown on this assumption by records of equally remarkable chromatic effects at both sunrise and sunset and about the sun at other times of the day prior to the eruption, it must be admitted at present that the volcanic eruption has strong claims to credence.

There can be no doubt that, whatever the prime cause, the effects are due to the presence in the higher regions of our atmosphere of a *form of matter* not usually there, at least to such an extent. Now this matter, or *form of matter*, may, as far as we know, be due to Krakatoa, to the earth's orbit traversing streams or regions pervaded with extremely fine meteoric dust, or to any other cause that might either introduce new or alter the form of existing matter.

It is well known in the laboratory that certain chemical combinations and mechanical mixtures will exist as such, but in a most unstable form,—a concussion or sharp sound, an electric spark, &c., either breaks them up or brings about a change of form so as to present altogether different physical properties. Now it is also well known that at the time of the Krakatoa eruption barometric pressure was spasmodically affected all over the world. Everywhere where barographs have been recorded this fact appears. This atmospheric shudder, undoubtedly originating at Krakatoa, was, I have reason to believe, conveyed rapidly from the centre through the higher and more tenuous regions of atmosphere, but affected the lower strata in its passage. This would perhaps account for the immense distance—thousands of miles—over which, it has been widely reported, explosions were heard about the time of the occurrence of the outburst.

Now if we assume that on the peripheral regions of our atmosphere *gases* and *forms of matter* exist in not very stable combinations or mixtures, it requires no great stretch of our imagination to picture the result of this great atmospheric shudder bringing about an alteration in the form or proportion of matter, and consequently such a change in its optical properties as to produce the unusual and remarkable effects which have been so universal.

ROBT. J. ELLERY

Melbourne Observatory, February 27

UNDER date of January 14 I named the bark *C. Southard Hurlburt* as having observed the glow on September 3. She was dismasted in a cyclone August 8, and came to Honolulu for repairs. On the former date she was in about lat. 17° N., long. 125° W. The captain's wife, Mrs. Davis, described the phenomena to me as extremely brilliant.

Only last week I learned from Hon. H. M. Whitney, Postmaster-General, that on September 5 Mrs. Whitney and himself distinctly observed the sun's disk before setting to be *green*. His residence was an exception to most of ours in Honolulu, from which trees cut off a view of the horizon. My wife spoke much that night of a strange green cumulus, seen by her ten minutes before calling me to observe the portentous masses of colour pouring out all over the sky.

I beg special attention to my former remark of the "earth-shadow sharply cutting off" the upper rim of the first glow. This was very manifest in the strong heavy glows of September, showing clearly that the first glow directly reflected the sun's rays, while in the after-glow which had no defined upper rim, but continued much longer, the haze reflects only the light of the first glow. This bears on estimates of the height of the haze.

Observers here are well agreed that during November there was a very great abatement of the glows, amounting almost to a cessation, although the whitish corona was always well developed through the day. Early in December the glows were renewed, and for six weeks continued quite as brilliant as during October. They are now somewhat abated, although quite uniform nightly. In September and October they were extremely unequal, as well as varying in position of greater colour, south or north of west.

As this revival of our glows closely followed their general diffusion over Europe and the United States, I suggest that this was the arrival in force by slow marches of the main body constituting the great *cone* of vapours, which, falling into the atmosphere in September, covered like a pall the Indian Ocean and Peninsula, down the extended western slope of which cone the light upper vapours were sent by the westward thrust of the earth's rotation, to find speed in their downward slide to carry them at once around the tropical belt as a light advance guard (as set forth in my letter of January 14). As the September haze became gradually dissipated, so the later December arrivals are wasting away.

S. E. BISHOP

Honolulu, January 30

AT Fanning's Island, long. 159° 22′ W., lat. 3° 52′ N., on September 4 last, the proprietor, Mr. Greig, states that the sun and sky had an extraordinary appearance; the sun "looked like a copper kettle." Lurid colours covered the sky. Great fears were felt for the safety of his schooner, the *Jennie Walker*, which sailed three days before.

From the master of the *Jennie Walker* I learn that on Sep-

tember 4 he was in long. 155° 28′ W., lat. 8° 20′ N., sailed from Fanning's Island three days before. At 5 p.m. noticed strange appearance in the sun, which was greenish. Strange colours over the west and around the sky at sunset. The sun was green at setting. Thought bad weather was portended. Never saw such appearances before.

Both parties are positive that the schooner was *three* days out when their fears were thus excited. She sailed September 1. No entry in ship's log of the above phenomena.

Honolulu, February 20 — S. E. BISHOP

ANOTHER note relative to these phenomena:—"*August* 20, 1861.—Earthquake at Naples. At Castellamare the water is so discoloured, that although the calm has been complete, we fear some subterranean perturbation. The heat is intense. At the same time the atmosphere presented a very peculiar appearance. There are no clouds, and during the whole week a thick mist has enveloped the city and coast, and *the sun when setting is as red as blood*."—(*Moniteur* du 29 Août, 1861.)

J. P. O'REILLY

Royal College of Science for Ireland,
Stephen's Green, Dublin

Meteorological Bibliography

I REJOICE to see the well-earned tribute which you have paid to Dr. Hellmann's excellent "Repertorium der Deutschen Meteorologie," and as a worker in the same field I trust that you will permit me to add that I agree with every word which your reviewer has said as to its excellence.

My object in writing is merely to point out that, thanks to the liberality of the United States Government, we may hope soon to have, not a perfect catalogue, but one which will be extremely useful, especially if, as I hope, the United States Government adopt my suggestion and endeavour to arrange with Dr. Hellmann for the incorporation of the first part of his "Repertorium" with the materials already forwarded to them.

Your reviewer is perfectly right in urging the absolute necessity of steps being taken to index and classify the multitudinous publications now appearing. Dr. Hellmann in his "Repertorium" says that 800 publications upon meteorology appear annually, or more than two each day, hence the impossibility of any one keeping abreast of the entire literature.

As regards the catalogue which I had the pleasure of sending to the United States last autumn, I annex an abstract of the description which I gave at the Southport meeting of the British Association in case you may think it of sufficient importance to be worthy of a place in NATURE. Dr. Hellmann's "Repertorium" only reached me just before my catalogue was shipped, hence the absence of reference to it in the annexed paper.

62, Camden Square, N.W., April 4 — G. J. SYMONS

On the Completion of the European Portion of the Preliminary Meteorological Catalogue, by G. J. Symons

The author commenced by giving a few illustrations of the large amount of time and energy which has been wasted by meteorologists, owing to their not knowing what had previously been done, sometimes even in their own country, but most frequently in other parts of the world; and he pointed out that with the modern development of meteorological work and of meteorological literature, some effort, upon a large scale, to deal with this evil was imperatively necessary.

Mr. Symons described the catalogue which he had formed during the last twenty or twenty-five years, by extracting (from many thousand catalogues issued by dealers in second-hand books in most of the capitals of Europe) all the titles of works on meteorology or kindred subjects. He also described the important publication, by the Royal Society, of its "Catalogue of Scientific Papers," and showed wherein the two agreed, and how largely each supplemented deficiencies in the other.

He then explained the steps which Prof. Cleveland Abbe had taken in preparing his card catalogue, and the arrangements whereby a copy of Mr. Symons's catalogue was to be prepared and forwarded to the United States for incorporation with Prof. Abbe's.

Mr. Symons then stated the additions which had been made to the original proposal, and that the following catalogues had been subsequently incorporated, each giving *approximately* the number of titles set against it, viz.:—

Prof. A. Poëy's	MS.	6000
Ronald's	Printed	3000
Struve's Pulkova	Printed	3000
Meteorological Society	MS. and printed ...	2000
Houzeau's Belgian	Printed	2000
Soc. Météor. de France ...	MS.	1500
Royal Observatory, Greenwich	MS.	500
Fineman's Swedish...	Printed	500
	Total about	18,500

Of course a great many of these were duplicates, but every catalogue contained titles which were not in any of the others, and altogether they have undoubtedly added very largely to the value of the work; it is impossible to state how largely, nor is it material in a case wherein the assistance rendered has been almost as great as the catalogue, and as diverse as the languages dealt with.

The precise number of titles forwarded is not known, but is probably about 20,000. Prof. Abbe's catalogue is understood to contain about 10,000, but probably there will be a few thousand common to both catalogues, and therefore the preliminary catalogue, which the United States Signal Office, under the direction of General Hazen, will proceed to prepare for publication, will probably contain the titles of more than 25,000 books and papers upon meteorology.

Mr. Symons remarked, in conclusion, that the catalogue must not be regarded as complete. It was impossible to make it perfect—it could not be perfect as regarded the past until every public and private library in the world had been searched. It could not be perfect for the present, because every day new works appeared in different parts of the world, and all could not be simultaneously inserted. Nor would his part of it bear bibliographical criticism, for he was not a bibliographical expert, and his chief aim had been to give information useful to working meteorologists.

Ice Volcanoes—Mountain Rainbow

THE past winter has been unusually cold and stormy in Ontario, and, as a result, an uneven strip of ice 100 to 200 yards wide has accumulated along the lake shore, sometimes forming mounds twenty or thirty feet high. Many of these mounds are conical, and have a crater-like opening communicating with the water. In stormy weather every wave hurls a column of spray and ice fragments through the opening. The ejecta freeze fast as they fall, and add to the height of the cone. In high winds the coast seems fringed with miniature volcanoes in active eruption. After a time the crater becomes clogged with ice, and the volcano may be looked on as extinct. Often a second crater is formed just to seaward of the first, and growing upon its ruins.

Mr. J. A. Fleming mentions in your issue for January 31 (p. 310) a circular rainbow seen from a hill-top against mist. I saw the same phenomenon three years ago near the Lofoden Islands, as a fog was breaking. It was noticed and admired by other passengers on the steamer also. Each saw his shadow enlarged upon the mist, and with the head surrounded by a brightly coloured halo or rainbow. The beautiful sight disappeared after a few minutes as the fog thickened again.

A. P. COLMAN

Faraday Hall, Victoria University, Cobourg, Canada

Thread-twisting

IN reply to "Cosmopolitan's" question in NATURE (vol. xxix. p. 525), I have been many years in Orkney, but do not remember to have seen the women twisting thread with "the palm of the hand on the thigh," but the fishermen there twist the short lengths of horsehair line called "snoods," which when united together form fishing lines of different strengths, in this manner.

The women of the North-American Indians always twist the short threads of sinew with which moccasins and leather clothes are sewn in this way: The sinew is torn up or divided into thin filaments slightly moistened by being drawn between the lips, then twisted between palm and thigh. — J. RAE

Kensington, April 5

IN reply to "Cosmopolitan's" query as to the occurrence of the habit of thread-twisting with the palm of the hand on the

thigh in other lands than India, I may say that I have observed the same mode of operating upon paper in Japan very frequently. The paper used there is tough and fibrous, and a Japanese is never at a loss for card to tie a parcel with if he has paper beside him. I have seen the spindle-whorl in actual use in upland districts, and it was employed even in Tokio very recently. HENRY FAULDS

Laurel Bank, Shawlands, Glasgow, April 7

Colony of Cats

IT may interest those of your readers fond of cats to know that a colony of cats live and breed under the wooden platform of the Victoria Station of the District Railway. They may be seen crossing the rails right in front of trains, and considering the enormous traffic, and the consequent noise and vibration, it certainly does seem remarkable that such naturally timid animals as cats should live amidst such unnatural surroundings. It may tend to show the plasticity of the animal creation generally in adapting itself to surrounding conditions. A female cat may have taken refuge there originally, and hence the railway domestication of the animals. GEORGE RAYLEIGH VICARS

London

Earthworms

SEEING the correspondence on this subject, I am led to give the following fact, which affords a further proof of the necessity of a vegetable deposit being formed previous to the existence of earthworms as stated by Mr. Melvin (vol. xxix. p. 502). A field two years ago was converted into a garden, and on account of bad cultivation, and by reason of each crop being altogether removed for several years in succession, no worms were there, but after the application of a large quantity of stable manure worms have appeared by hundreds, and their castings after rain afford ample proof of their activity. TRANSformation of vegetable mould combined with animal refuse into available food for plants is here made evident. J. LOVELL

Driffield, April 7

"The Axioms of Geometry"

PROF. HENRICI, in NATURE, vol. xxix. p. 453, considers Hamilton's proof of Euclid I. 32 invalid; and asserts that from his reasoning it would follow that the sum of the three angles of a spherical triangle equals two right angles. I venture to differ from him for the following reason:—The only thing which Hamilton requires to be granted is that when a moving *straight* line slides along a *fixed* straight line its *direction* is unchanged. This axiom will, I suppose, be granted by every one. Of course it is not true that in every case rotation is independent of translation. But Hamilton's proof does not require it to be true in every case, but only in the case of a *straight* line. Hence I maintain that Hamilton's reasoning is perfectly correct, and his proof valid. EDWARD GEOGHEGAN

Bardsea, March 26

GEOLOGY OF CENTRAL AFRICA

THE following extract from a letter received by Mr. Geikie from Mr. Henry Drummond, who is at present exploring the Lake region, may interest our readers:—

"*Maramoura, Central Africa, November* 1, 1883

"I have now completed a traverse from the mouth of the Zambesi, by way of the Shire highlands, in a north-west direction, until the line joins Mr. Joseph Thomson's route, about half way between Lakes Nyassa and Tanganyika. I have filled in the geology so far as is possible in a single survey, and hope thus to be able to extend the sketch geological map, begun by Thomson, for some distance south and west. I may still further extend this by an expedition to Lake Bangweolo, after the rainy season, but there are circumstances which may make it necessary for me to leave for home in February or March. Perhaps the most interesting thing I have to note is the discovery here of a small but rich bed of fossils. The strata alluded to consist of light coloured limestones and shales, with beds of fine gray sandstones, and the fossils include plant, fish, and molluscan remains. Plants are the most scarce, but fish-scales and teeth exist in vast numbers. Unfortunately whole fish are extremely rare, and after three or four days' search I have only succeeded in securing two or three indifferent specimens. The mollusks, on the other hand, are obtainable in endless quantity, and are in fine preservation. Indeed there is one small bed of limestone entirely made up of these remains, all, however, belonging to a single species. From the general character of the beds I am inclined to think they are of lacustrine origin. These fossiliferous beds are *the only sedimentary rocks* I have crossed between the mouth of the Shire—say 130 miles from the coast—and the centre of the Nyassa-Tanganyika plateau. At the point where I crossed them they are not more than a couple of miles in breadth, and are flanked on either side by granite and gneiss. They lie at a short distance from Lake Nyassa, and are probably part of the Mount Waller series. This series stretches for some short distance along the north-west shore of the lake, but is apparently of no great extent. These deposits may possibly throw some light on the problem of the lake.

"As regards the controversy between Mr. Thomson and Mr. Stewart about (1) the Livingstone Mountains, and (2) the bed of iron between the lakes, I should say that on both points both explorers are right from their own point of view.

"Mr. Stewart had only been dead a few days when I reached the north end of Nyassa. It was a great disappointment and blow to me, as I looked forward to much help from him. No one living possesses anything like his knowledge of the physical geography of this part of the interior."

CHINESE PALÆONTOLOGY

PALÆONTOLOGY is not a study that commends itself to the attention of Chinamen. With archæology the case is different. That is a pursuit which within historical limits the Chinese follow with enthusiasm. Every one who possesses any pretensions to culture, and who can afford to indulge the inclination, collects all that is old from cracked china to coins. So prevalent is this taste, and so keen is the competition for objects bearing the stamp of age, that a flourishing trade, such as rivals the celebrated traffic in "antiquities" carried on at Jerusalem, exists in fabricated antiques for the benefit of inexperienced native collectors and foreign purchasers. But natural antiquities are, speaking generally, left unnoticed, or if thought of for a moment are hastily explained by random conjectures. Topsy's celebrated explanation of her existence is about on a par with the guesses which are hazarded by the most learned Chinamen to account for palæontological phenomena. Science has always a borderland of unsolved questions, but in China this borderland exceeds in extent the territory of knowledge in the possession of the people. They have no aptitude for palæontology, and few writers make any reference to it. Among the rare exceptions to this rule is Ch'ên Kwah of the Sung Dynasty (A.D. 960-1127), who, in an interesting work entitled "Notes from a Dreamy Valley," has collected a number of facts on natural antiquities as well as on other matters. His knowledge is not deep, but when we remember that Voltaire accounted for the presence of marine shells on the top of the Alps by supposing that pilgrims in the Middle Ages had dropped them on their way to Rome, a great deal may be forgiven a Chinese writer of the eleventh century.

The Chinese have so completely lost sight of the possibility of the existence in China of any civilisation but their own that when they meet with traces of earlier man they attribute them either to blind chance or to

supernatural causes. In this way when Ch'ên Kwah met in the course of his investigations with flint and bronze implements he at once adopted the common opinion of his countrymen, which is the same as that which was prevalent in Europe a couple of centuries ago, that they were thunderbolts shot down by the God of Thunder in the explosions of his wrath. In confirmation of this theory Ch'ên states that though these implements are found all over the country they are more plentiful in districts, such as Lui-chow in the province of Canton, where thunderstorms are more than usually prevalent. In shape, he tells us, they resemble axes, knives, small hammers several pounds in weight, skewers or nails, and other pointed implements. In colour they vary, some being yellow, some green, and some black. Some of the axe-shaped stones are bored with two holes, but the majority are not pierced, and implements of the same shape are found in bronze and iron.

Speaking within his own knowledge he only describes the circumstances of the discovery of two stone axes, both of which he tells us were found beneath trees. In one case, at Sin-chow, in Hupeh, after a severe thunderstorm in which, like Prospero, the God of Thunder had

"rifted Jove's stout oak,
With his own bolt,"

a stone axe was found at its roots; and on another occasion at Sui-chow, under precisely similar circumstances, a shepherd-lad found a "fire stone in the shape of an axe." As in the only two cases about which Ch'ên speaks from personal knowledge the axes were found beneath trees, it is not unnatural to suppose that they are more frequently found in that position than elsewhere; and this becomes interesting when we find it stated by Mr. Rivett Carnac in a valuable paper published in vol. lii. of the *Proceedings* of the Bengal Branch of the Royal Asiatic Society, that it is the custom in Central India for the finder of a stone axe or other stone implement to place it "under the village pipul tree," and sometimes to sanctify it with a daub of red paint, and thus to constitute it a Mahadeo. A somewhat similar practice exists, according to Chinese historians, in a country vaguely described as being to the west of the Yuh Pass in Chinese Turkestan, where "thunder stones" when found are deposited in the temples. May not this Indian practice have also been the custom of some of the aboriginal tribes of China? and may not the fact that in the two instances mentioned above the axes were found at the roots of riven trees be evidence of the antiquity of the custom, as in cases described by Mr. Rivett Carnac, in which the roots of the trees and the surrounding soil had in course of years so completely grown over the axes that they could only be cut and dug out with difficulty?

Stone arrow-heads do not seem to have come within Ch'ên's range of observation, although from historical references we know that they are to be met with in China. In the "Book of History," which is said to have been compiled by Confucius, mention is made of tribute, consisting of iron, silver, steel, and stone arrow heads, having been presented to the Chinese Court by the tribes on the Yellow River about the year 2200 B.C. The story is told also that on one occasion, as the Prince of Ch'ên (495 B.C.) was walking in the palace grounds, a bird fell dead at his feet, pierced through by a stone-headed arrow. As the kind of bird was unknown to the prince and his courtiers, Confucius was called in to give his opinion upon it. The bird he pronounced to be a species of sparrow-hawk from Northern Tartary, and he explained that the stone head which pointed the dart was similar to that which Wu Wang (B.C. 1122) presented to his prince. It appears also that stone arrow-heads were used in ancient times as emblems of authority, and that they have very commonly been presented to sovereigns as objects of curiosity and value.

The biographical dictionaries tell us that in course of his official duties Ch'ên was called upon to direct extensive irrigating works; and no doubt the excavations and cuttings which he then superintended led him to take an interest in the fossil remains with which the country abounds. On this subject he has many notes. In one he tells us that at a certain spot on the Yellow River, the banks having fallen away for a considerable distance, a fossil bamboo grove was disclosed, a fact which excited his surprise, as the district is not one in which bamboos grow at the present day, and he contrasts with this the fossil peach-stones, roots of rushes, snakes and crabs, which are found at the Kin-hwa Mountain, all of which things are still indigenous in the neighbourhood. At Tsêh-chow in Shansi, he states, a man, when digging a well, suddenly unearthed a "lizard resembling a dragon." At sight of the monster the man fled in terror, but observing from a distance that it remained motionless, he ventured to return, when, to his relief, he found that it was petrified. Philistine-like, his neighbours broke it to pieces, and only one bit of it was preserved. Another kind of fossil has long been a puzzle to the philosophers, from the great and wise emperor, K'ang-hi (1661-1720), downwards. Adventurous travellers who have braved the northern frosts have from time to time brought back accounts of the mammoths which are found in the frozen cliffs of Siberia. Deceived by a mistaken analogy, the Chinese wiseacres have arrived at the conclusion that these monsters must be huge ivory-producing rats, and, misinterpreting their continued preservation, have formed the opinion that darkness is necessary to their life, and that exposure to the outer air produces instant death. Their ivory is considered to be softer than elephant ivory, and in the hands of skilful chemists their flesh is said to make up into a highly invigorating tonic.

Speaking of the neighbourhood of the Loh River, Ch'ên mentions the discovery of ancient Troglodyte dwellings in which were found coins, and in one case a stone chest bearing on the outside fine tracings of flowers, birds, and other objects. On the lid were inscribed upwards of twenty characters, which were of such an archaic form that they were undecipherable. But the contents were easily understood, and were at once recognised as pieces of pure gold.

Constant mention is made by Ch'ên of meteoric stones, which in popular imagination are said to assume various strange and uncanny forms. Of the descent of one such stone which fell in the province of Kiang-su in the year 1064, he gives certain particulars on the authority of a Mrs. Heu. This lady, when in her garden one day, was startled by an explosion like a peal of thunder, and saw a large "star nearly as big as the moon" pass across the sky from south-east to south-west, and eventually fall within a few yards of the place where she was standing. On going to the spot she observed a deep hole, at the bottom of which was the "star shining brightly." By degrees the light died away, and eventually at a depth of three feet she dug up a round stone of the size of a man's fist, and of the weight and appearance of iron. Altogether Ch'ên's work is well worthy of the study of those who can read Chinese and who are interested in the palæontology of China.

ROBERT K. DOUGLAS

ON THE FORMATION OF STARCH IN LEAVES

IN a recent communication to the *Arbeiten des botanisches Institut* in Würzburg (Bd. iii.), Prof. Sachs gives the results of his work during the past summer in connection with the above subject. The investigations were made with the object of determining the formation and disappearance of starch in the leaves of plants growing in the open, and under normal conditions of vegeta-

tion, and were carried on chiefly during the months of June, July, and August on a large number of Dicotyledons from various families. Some twenty-two years ago Prof. Sachs showed that the presence of starch in chlorophyll grains can readily be detected by means of the now well-known iodine test, a modification of which was employed in these researches.

If fresh green leaves are plunged into boiling water for ten minutes or so, certain soluble substances are extracted, but the starch and colouring matter of the chlorophyll grains remain in the still unbroken cells of the mesophyll. A short immersion in alcohol now removes the green colouring-matter and certain bodies soluble in alcohol, leaving the starch behind in the colourless tissue. The presence of acids affects the degree of whiteness of the decolorised leaf; and the decolorisation proceeds more rapidly in sunlight or warm alcohol than in the dark and cold. Leaves of *Tropæolum* may be rendered completely white, like writing paper, in two or three minutes.

If the decolorised leaf be now placed in a strong solution of iodine in alcohol, the presence or absence of starch may be demonstrated in a few minutes. If no starch is present, the cellular tissue simply presents the well-known yellow colour; if a large quantity of starch exists in the cells, the tissue appears blue-black, the venation appearing as a pale network in the dark ground. Paler colours result if but little starch is present at the time of the experiment.

It will readily be seen how useful the above method is for the purpose of demonstrating the absence of starch from etiolated leaves, the white portions of variegated foliage, &c., and the sequel shows that the method affords means of obtaining far more delicate results, without the trouble of a microscopic examination.

In the first place, the same leaf may be found to contain very different quantities of starch at different periods of the day, or according to the weather; and secondly, the increase and decrease of the quantities of starch in a given leaf may be very rapid.

Sachs showed long ago that if a plant is placed in the dark, the starch disappears from the leaves; and it has also been known for some time that if a piece of tinfoil is placed on a leaf, the covered portion forms no starch, although the parts exposed to light may become filled with that substance. Moreover, Kraus showed how very rapidly starch can be formed in direct sunlight.

Sachs now demonstrates on a number of plants that the starch formed in the leaves during the day may disappear completely during the night, and that the leaves shown to be full of starch in the evening may be quite empty of starch next morning. This depends upon the temperature and health of the plant, but occurs normally during the summer in plants growing in the open. A large number of experiments are given in support of this, and showing how the rapidity and completion of the process depends upon the weather.

The experimental proof is very simple. A leaf is halved longitudinally at night, after a fine sunny day, and the excised half is shown to be filled with starch by the iodine test described; the remaining half is tested early next morning, and shows at once if any material diminution has occurred during the night. A simple and obvious modification of this experiment gives an idea of the quantity of starch formed between sunrise and sunset. The half leaf tested before sunrise shows no trace of starch: the other half, left on the plant during the day, is found to become more and more filled with starch towards the afternoon.

Some curious results were arrived at as to the effect of growing parts on the rapidity of the emptying of the leaves; some of these matters still require investigation.

Differences in the weight of leaves and in the intensity of the colour produced by the iodine test, as well as some other observations, lead to a better understanding of a fact already known generally, viz. that the starch disappears from the leaves in the form of glucoses, which travel by way of the vascular bundles into the stems, and thus pass to the places where they are used up in growth.

Some very telling observations were made in this connection, and the dependence of the processes on temperature again show forth clearly.

These results lead to the conclusion that the process of metamorphosis into glucoses and translocation of the products of assimilation are also going on during daylight, though they are less evident, because more starch is then being formed and accumulated than is abstracted at the time. Moll proved that such is the case by exposing leaves to the sunlight, but in an atmosphere devoid of carbon dioxide; the starch already in the leaves disappeared, and no more was formed to replace it. Sachs repeated Moll's experiments, and proved the correctness of his conclusions by means of the iodine test. Half leaves were shown to be full of starch; the companion halves were put into closed atmospheres, deprived of carbon dioxide by means of potassium hydrate, and exposed to sunlight. In an hour the latter halves were tested, and found to be nearly emptied of starch. Other experiments proved that depletion occurred in a few hours, the time depending on the temperature.

Further experiments demonstrate that the starch travels in the form of glucoses in all the above cases; but it is not proved whether the metamorphosis is effected by forces in the chlorophyll grains themselves, or by means of diastatic ferments in the cells of the leaf. A few hints are here given showing a field for further research.

Perhaps the most ingenious part of the paper is that which now follows. It is well known that Weber's patient and thorough researches on the energy of assimilation led to two important results, among others: (1) that the quantity of starch formed by a certain area of leaf-surface in a certain time may be relatively very large; and (2) that different plants probably differ specifically as to the quantities of starch formed in their leaves.

Sachs proposes to apply his method to the solution of this question, *i.e.* how much starch is produced in, say, one square metre of leaf-surface by assimilation during, say, ten hours' bright sunlight? The great difficulties in Weber's researches were connected with the enormous labour necessary to measure the leaf-surface accurately.

Sachs resolved the matter in a manner which we may summarise thus:—He cut off portions of large leaves found to be empty of starch, measured them rapidly by laying them on pieces of board cut to the size of one square metre, and killed, dried, and weighed the measured portions very rapidly. Certain precautions as to the area of fibro-vascular bundles, the possibilities of absorbing hygroscopic moisture, &c., may here be passed over. Supposing these portions of the leaves to be estimated in the morning, a quantity of the same leaves of equal area gathered in the evening was then compared, and the increase in weight gives the quantity of starch formed in the interval. By weighing large areas, and frequently, and by paying attention to the times and other circumstances, a large number of results were obtained, showing that the quantities given by Weber, for instance, are within the mark. Of course these results are not absolute. Starch is being changed into glucose, and passing away during the day, and some must be burnt off in respiration; moreover a certain minute quantity of mineral ash should be allowed for. Of course, it is an assumption that equal areas of mesophyll of the same leaves contain approximately the same amount of substance: nevertheless, if a large number of experiments are made, the error is probably small.

Experiments were made to show both the quantities of starch which disappear during the night and the quanti-

ties formed during the day. A few of the numbers may be given. In *Helianthus*, 9·64 grms. of starch disappeared in ten hours from one square metre of leaf-surface.

In the same plant 9·14 grms. were formed in the same time by the same area of leaf-surface.

In another case *Helianthus* was used, but the leaves were removed from the stem to prevent the passage back of the starch from the mesophyll into the stems.

A square metre was found to produce starch at the rate of 1·648 grms. per hour.

By combining his experimental results and taking note of all the circumstances, Sachs concludes that twenty to twenty-five grms. of starch per day may be produced by one square metre of leaf-surface as an ordinary occurrence; and these numbers are not only not excessive, but experiments show that there are plants which produce much more than those investigated here.

Some remarkably interesting and important results follow from the consideration of these experimental data.

They explain why plants are so vigorous during warm nights following upon hot bright days. The more readily the products of assimilation (formed in large quantities during the day) can pass into the growing organs, the better these are nourished, and so forth.

Leaves used for fodder, &c., must differ in nutritive value to a very great extent if their starchy contents vary so largely during the day and night: it thus becomes of primary importance whether such leaves are gathered in the morning or the evening, in cold or warm weather, &c. The same applies to *Tobacco* and *Tea*, &c. It must make a vast difference to the smoker whether his tobacco abounds in carbohydrates or is relatively richer in the alkaloids. It appears that tobacco is habitually cropped in the morning in some countries, a fact which suggests that experience has already shown that a difference in the quality exists; it will be interesting to inquire further into these matters.

Sachs's results will also materially affect the physiological value of the analyses of leaves. Some of us know how great are the variations met with in analyses of the ash contents of leaves of the same plant. It is clear that, in addition to the age of the leaf, the soil, manure, &c., it is important to know the amount of starch present. It cannot but happen that the mineral matters ebb and flow as well as the starch. The analyses of leaves will also be more valuable for the purposes of physiology if the numbers are stated, not in simple percentages, but in terms of one square metre of the leaf-surface.

The above brief summary of the results obtained by Prof. Sachs by no means does justice to the beauty of his methods, and the masterly way in which they were carried out: it must be admitted by all who understand the value and importance of this work that it is worthy of the great pioneer of vegetable physiology. Moreover, it suggests several matters which require further investigation, and would no doubt yield valuable results to those fortunate enough to have a botanical garden at hand.

H. Marshall Ward

Botanical Laboratory, Owens College

TELEPHONY AND TELEGRAPHY ON THE SAME WIRES SIMULTANEOUSLY

For the last eighteen months a system has been in active operation in Belgium whereby the ordinary telegraph wires are used to convey telephonic communications at the same time that they are being employed in their ordinary work of transmitting telegraphic messages. This system, the invention of M. Van Rysselberghe, whose previous devices for diminishing the evil effects of induction in the telephone service will be remembered, has lately been described in the *Journal Télégraphique* of Berne by M. J. Banneux of the Belgian Telegraph Department. Our information is derived from this article and from others by M. Hospitalier.

The method previously adopted by Van Rysselberghe, to prevent induction from taking place between the telegraph wires and those running parallel to them used for telephone work, was briefly as follows:—The system of sending the dots and dashes of the code—usually done by depressing and raising a key which suddenly turns on the current and then suddenly turns it off—was modified so that the current should rise gradually and fall gradually in its strength by the introduction of suitable resistances. These were introduced into the circuit at the moment of

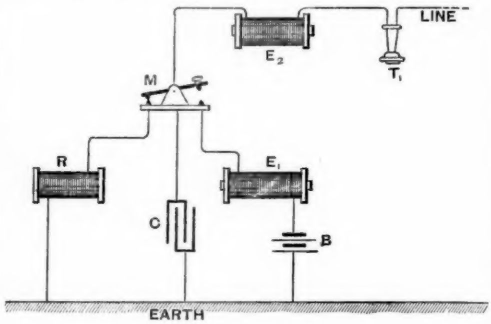


Fig. 1

closing or opening by a simple automatic arrangement worked exactly as before by a key. The result of the gradual opening and gradual closing of the circuit was that the current attained its full strength gradually instead of suddenly, and died away also gradually. And as induction from one wire to another depends not on the strength of the current, but on the rate at which the strength changes, this very simple modification had the effect of suppressing induction. Later Van Rysselberghe changed these arrangements for the still simpler device of introducing permanently into the circuit either condensers or else electromagnets having a high coefficient

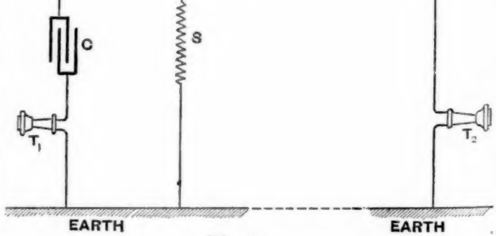


Fig. 2

of self-induction. These, as is well known to all telegraphic engineers, retard the rise or fall of an electric current; they fulfil the conditions required for the working of Van Rysselberghe's method better than any other device.

Having got thus far in his devices for destroying induction from one line to another, Van Rysselberghe saw that, as an immediate consequence, it might be concluded that, if the telegraphic currents were thus modified and graduated so that they produced no induction in a neighbouring telephone line, they would produce no sound in the telephone if that instrument were itself joined up in the telegraph line. And such was found to be the case

Why this is so will be more readily comprehended if it be remembered that a telephone is sensitive to the changes in the strength of the current if those changes occur with a frequency of some hundreds or in some cases thousands of times *per second*. On the other hand, currents vibrating with such rapidity as this are utterly incompetent to affect the moving parts of telegraphic instruments, which cannot at the most be worked so as to give more than 200 to 800 separate signals *per minute*.

The simplest arrangement for carrying out this method is shown in Fig. 1, which illustrates the arrangements at one end of a line. M is the Morse key for sending

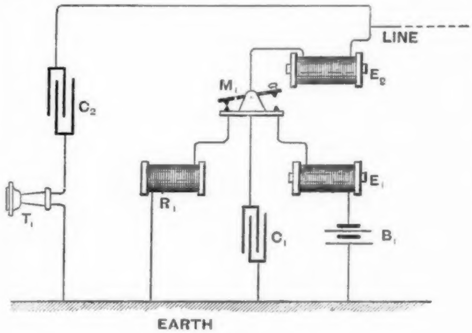


Fig. 3

messages, and is shown as in its position of rest for receiving. The currents arriving from the line pass first through a "graduating" electromagnet, E_2, of about 500 ohms resistance, then through the key, thence through the electromagnet R of the receiving Morse instrument, and so to the earth. A condenser, C, of 2 microfarads capacity is also introduced between the key and earth. There is a second "graduating" electromagnet, E_1, of 500 ohms resistance introduced between the sending battery B and the key. When the key M is depressed in order to send a signal, the current from the battery must charge the condenser C, and must magnetise the cores of the two electromagnets E_1 and E_2, and is thereby retarded in rising to its full strength. Consequently no sound is heard in a telephone, T, inserted in the line-circuit. Neither the currents which start from one end nor those which start from the other will affect the telephones inserted in the line. And, if these currents do not affect telephones in the actual line, it is clear that they will not affect telephones in neighbouring lines. Also the telephones so inserted in the main line might be used for speaking to one another, though the arrangement of the telephones in the same actual line would be inconvenient. Accordingly M. Van Rysselberghe has devised a further modification in which a separate branch taken from the telegraph line is made available for the telephone service. To understand this matter one other fact must be explained. Telephonic conversation can be carried on even though the actual metallic communication be severed by the insertion of a condenser. Indeed, in quite the early days of the Bell telephone, an operator in the States used a condenser in the telegraph line to enable him to talk through the wire. If a telephonic set at T_1 (Fig. 2) communicate through the line to a distant station, T_2, through a condenser, C, of a capacity of half a microfarad, conversation is still perfectly audible provided the telephonic system is one that acts by induction currents. And since in this case the interposition of the condenser prevents any continuous flow of current through the line, no perceptible weakening will be felt if a shunt, S, of as high a resistance as 500 ohms and of great electro-magnetic rigidity, that is to say, having a high coefficient of self-induction, be placed across the circuit from line to earth. In this, as well as in the other figures, the telephones indicated are of the Bell pattern, and if set up as shown in Fig. 2, without any battery, would be used both as transmitter and receiver on Bell's original plan. But as a matter of fact any ordinary telephone might be used. In practice the Bell telephone is not advantageous as a transmitter, and has been abandoned except for receiving; the Blake, Ader, or some other modification of the microphone being used in conjunction with a separate battery. To avoid complication in the drawings, however, the simplest case is taken. And it must be understood that instead of the single instrument shown at T_1 or T_2 a complete set of telephonic instruments in-

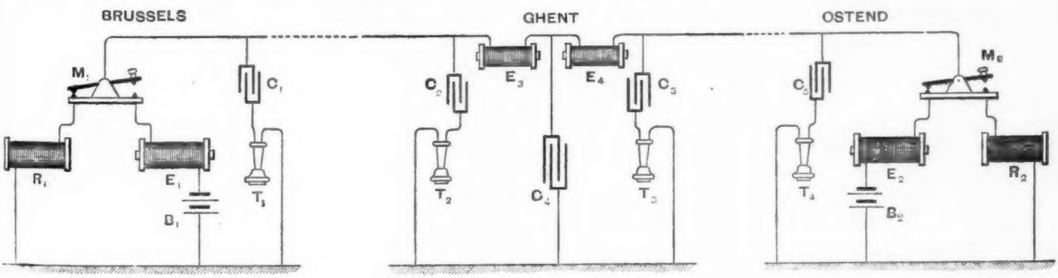


Fig. 4

cluding transmitter, battery, induction-coil, and receiver or receivers, may be substituted. And if a shunt, S, of 500 ohms placed across the circuit makes no difference to the talking in the telephones because of the interposition of the separating condenser C, it will readily be understood that a telegraphic system properly "graduated," and having also a resistance of 500 ohms, will not affect the telephones if interposed in the place of S. This arrangement is shown in Fig. 3, where the "graduated" telegraph-set from Fig. 1 is intercalated into the telephonic system of Fig. 2, so that both work simultaneously, but independently, through a single line. The combined system at each end of the line will then consist of the telephone-set T_1, the telegraph instruments (comprising battery B_1, key M_1, and Morse receiver R_1), the "graduating" electromagnets E_1 and E_2, the "graduating" condenser C_1, and the "separating" condenser C_2. It was found by actual experiments that the same arrangement was good for lines varying from 28 to 200 miles in length. A single wire between Brussels, Ghent, and Ostend is now regularly employed for transmission by telegraph of the ordinary messages and of the telemeteorographic signals between the two observatories at those places, and by telephone of verbal simultaneous correspondence

for one of the Ghent newspapers. A still more interesting arrangement is possible, and is indicated in Fig. 4. Here a separating condenser is introduced at the intermediate station at Ghent between earth and the line, which is thereby cut into two independent sections for telephonic purposes, whilst remaining for telegraphic purposes a single undivided line between Brussels and Ostend. Brussels can telegraph to Ostend, or Ostend to Brussels, and at the same time the wire can be used to telephone between Ghent and Ostend, or between Ghent and Brussels, or both sections may be simultaneously used.

It would appear then that M. Van Rysselberghe has made an advance of very extraordinary merit in devising these combinations. We have seen in recent years how duplex telegraphy superseded single working, only to be in turn superseded by the quadruplex system. Multiplex telegraphy of various kinds has been actively pursued, but chiefly on the other side of the Atlantic rather than in this country, where our fast-speed automatic system has proved quite adequate hitherto. Whether we shall see the adoption in the United Kingdom of Van Rysselberghe's system is, however, by no means certain. The essence of it consists in retarding the telegraphic signals to a degree quite incompatible with the fast-speed automatic transmission of telegraphic messages in which our Post Office system excels. We are not likely to spoil our telegraphic system for the sake of simultaneous telephony, unless there is something to be gained of much greater advantage than as yet appears.

NOTES

WE are pleased to be able to announce that Prof. Flower's title is to be "Director" of the Natural History Museum, South Kensington, not "Superintendent," as Prof. Owen was styled. According to the Civil Service Estimates for the present financial year his staff consists of four keepers of departments (Botany, Geology, Mineralogy, and Zoology), two assistant keepers (Geology and Zoology), eleven first-class assistants, and fourteen second-class assistants. Large as this number may seem, it is notorious that in the Zoological Department at least a considerable reinforcement is required before the work can be expected to be efficiently performed.

WE regret to learn from the *Times* that M. Dumas, the venerable *Secrétaire perpetuel* of the Academy of Sciences, is lying in a critical state at Cannes.

POPE LEO XIII. has erected at his own expense at Carpineto-Romano, his native city, a meteorological observatory. It has been placed at the top of the castle of the Pecci family. The directorship of this establishment, which will be one of the most important in the whole Italian system, has been given to Count Lodovico.

WE are pleased to receive the first official publication issued from the Hong Kong Observatory by Dr. Doberck, giving the results of observations during the month of January. We are sure the establishment of this institution will be of the greatest service both to navigation and to science.

THE first International Ornithological Congress ever held was on Monday festively inaugurated at Vienna by its patron, the Crown Prince Rudolph—himself a noted ornithologist. In his opening speech, the Prince dwelt upon the great importance of those studies in natural history which characterise this century, a remark which was doubtless meant as a reply to the vehement attacks on modern science recently made by the Clerical Deputy Greuter in the Austrian Parliament. Germany and Austria have sent hither all their ornithological celebrities; but the Congress also includes delegates from the Russian and French Governments, and members from Switzerland, Holland, and Sweden. Even Siam and Japan are represented, while England is conspicuous by her absence. The Congress began its deliberations with the question of International Protective Legislation for Birds.

THE sixth Archæological Congress will be held at Odessa from August 27 to September 1.

A SOMEWHAT novel feature in connection with the International Health Exhibition this year will be the establishment of a library and reading-room, a home for which the executive council have assigned in a large double room in the Albert Hall, overlooking the conservatory. Steps have been taken to secure a representative collection of works on vital statistics; of reports and regulations relating to public health; of regulations with reference to injurious trades and of works thereon; and of reports, statistics, and other works on the science of education. Foreign powers have been invited to lend their cooperation in this effort to create an international library of works of reference bearing on the two divisions of the Exhibition, and several responses have already been received. India and the Colonies have also been asked to contribute towards the same end. Publishers and authors have likewise been invited to forward copies of their works. In addition to the library of reference, there will be a reading-room, to which the current numbers of periodical publications of a sanitary or educational character will be admitted. All books and periodicals sent to the library and reading-room will, under certain regulations, be arranged for the use of visitors, and not merely for exhibition. The books will be submitted to the jurors, and a full catalogue will be issued. All parcels for the library and reading-room should be addressed, carriage paid, to the Secretary of the Library Sub-Committee, Royal Albert Hall, London, S.W. The following handbooks are being written in connection with the Exhibition:—"Healthy Villages" (illustrated), by H. W. Acland, C.B., M.D., F.R.S.; "Healthy Bed-Rooms and Nurseries, including the Lying-in Room," by Mrs. Gladstone; "Healthy and Unhealthy Houses in Town and Country" (illustrated), by Mr. W. Eassie, C.E., with an appendix by Mr. Rogers Field, C.E.; "Healthy Furniture and Decoration" (illustrated), by Mr. R. W. Edis, F.S.A.; "Healthy Schools," by Mr. Charles Paget, M.R.C.S.; "Health in Workshops," by Mr. J. B. Lakeman; "Manual of Heating, Lighting, and Ventilation" (illustrated), by Capt. Douglas Galton, C.B., F.R.S.; "Food," by Mr. A. W. Blyth, M.R.C.S.; "Principles of Cookery," by Mr. Septimus Berdmore; "Food and Cookery for Infants and Invalids," by Miss Wood, with a preface by R. B. Cheadle, M.D., F.R.C.P.; "Drinks, Alcoholic," by John L. W. Thudichum, M.D., F.R.C.P.; "Drinks, Non-Alcoholic and Aërated," by John Attfield, Ph.D., F.R.S.; "Fruits of all Countries" (illustrated), by Mr. W. T. Thiselton Dyer, M.A., C.M.G.; "Condiments, including Salt," by the Rev. J. J. Manley, M.A.; "Legal Obligations in respect to Dwellings of the Poor," by Mr. Harry Duff, M.A., Barrister-at-Law, with a preface by Mr. Arthur Cohen, Q.C., M.P.; "Moral Obligations of the Householder, including the Sanitary Care of his House," by G. V. Poore, M.D., F.R.C.P.; "Laboratory Guide to Public Health Investigation" (illustrated), by W. W. Cheyne, F.R.C.S., and W. H. Corfield, M.D., F.R.C.P., M.A.; "Physiology of Digestion and the Digestive Organs," by Prof. Arthur Gamgee, F.R.S.; "Fermentation," by Dr. Duclaux, with a preface by M. Louis Pasteur, Membre de l'Institut; "Spread of Infection," by Mr. Shirley F. Murphy; "Fires and Fire Brigades" (illustrated), by Capt. Eyre M. Shaw, C.B.; "Scavengering and other such Work in Large Cities," by Mr. Booth Scott; "Athletics," Part I. (illustrated), by the Rev. E. Warre, M.A.; "Athletics," Part II., by the Hon. E. Lyttleton, M.A., and Mr. Gerard F. Cobb, M.A.; "Dress in relation to Health and Climate" (illustrated), by Mr. E. W. Godwin, F.S.A.; "The

Ambulance" (illustrated), by Surgeon-Major Evatt, M.D., A.M.D.; "The Influence of Schools of Art on Manufacturing Industry," by John Sparkes; "The Homes of the Poor," (author not yet settled).

LADY SIEMENS has placed at the disposal of the Council of the Society of Arts a sum of 20*l.*, to provide a prize, to be called the Siemens Prize, to be offered for the best application of gas to heating and cooking in dwellings (Class 24 in the International Health Exhibition). The prize will consist of a gold medal or 20*l.*, and will be awarded under the same conditions as the prizes announced in the *Journal of the Society of Arts* of the 14th inst.

THE Senkenberg Natural History Society at Frankfort has had a legacy of 40,000*l.* left to it by the late Countess Bose.

A HUMAN skull has just been discovered in a bed of clay near Podbaba in the neighbourhood of Prague. A few days previously a mammoth tusk was found in the same locality. The colour of the skull proves that it was lying in yellow diluvial loam. It is remarkable on account of its very flat forehead and the thickened eyebrow bones, thus closely approaching the well-known Neanderthal skull. Its facial angle seems to be even smaller than that of the latter, although an exact measurement is impossible on account of the absence of part of the jaw-bones. Further details on the subject will be published in the *Transactions of the Bohemian Academy of Sciences.*

THE first number has just been issued of a new Italian quarterly, entitled *La Nuova Scienza, Rivista dell' Istruzione Superiore*, edited by Prof. Enrico Caporali of Todi, Umbria. As implied by the title, the aim of this periodical is to popularise scientific subjects, and to chronicle the progress of discovery in Italy and abroad. The editor invites communications in the chief European languages, and declares that his efforts will be mainly directed to promoting the unification of the sciences with a view to the ultimate constitution of an exact philosophy. To the present number he contributes two spirited and learned papers on "Modern Thought in Italy," and on "The Pythagorical Formula of Cosmic Evolution." The appearance of such a publication in a small provincial town is itself a striking illustration of the general revival of serious studies since the establishment of political unity in Italy.

THE much discussed question as to the purification of water in rivers "by itself," that is, by the mere fact of its motion, seems to have entered into a new phase. Dr. Pehl, at St. Petersburg, has recently made a series of bacterioscopic measurements on the waters of the capital, which are summed up in the last issue of the *Journal of the Russian Chemical Society.* The water of the Neva itself appears to be very poor in bacteria, namely, 300 germs in a cubic centimetre. After heavy rains this number rises to 4500, and to 6500 during the thawing of the river. The canals of St. Petersburg, on the contrary, are infected with bacteria, their number reaching 110,000 in a cubic centimetre, even during good weather. The same is true with regard to the conduits of water for the supply of the city. While its chemical composition hardly differs from that of the Neva (by which they are supplied), the number of bacteria reaches 70,000, against 300 in the water freely taken from the river; and the worst water was found in the chief conduit, although all details of its construction are the same as in the secondary conduits. Dr. Pehl explains this anomaly by the rapidity of the motion of water, and he has made direct experiments in order to ascertain that. In fact, when water was brought into rapid motion for an hour, by means of a centrifugal machine, the number of developing germs was reduced by 90 per cent. Further experiments will show if this destruction of germs is due to the motion of the mass of water, or to molecular motion. The germs, among which Dr. Pehl distinguishes eight species, are not killed by immersion into snow. As the snow begins to fall it brings down a great number of germs, which number rapidly diminishes (from 312 to 52 after a three hours' fall of snow, on January 21, 1884), while their number on the surface of the snow increases, perhaps in consequence of the evaporation of snow or of the condensation of vapour on its surface.

IT is proposed, *Science* states, to establish a monthly *American Meteorological Journal.* It will begin with from twenty-four to thirty-two octavo pages, and will be enlarged as rapidly as is justified by the support given it. The first number will probably appear about May 1. It will be published in Detroit by Dr. W. H. Burr, and edited by Prof. M. W. Harrington of Ann Arbor.

IT is stated that the earthquakes of March 25 in Southern Hungary were also severely felt at Essegg, at Winkowze, and at Fünfkirchen. At Djakovar many houses were injured. Another earthquake was remarked at Ischia on March 28. The shock was but a slight one and of short duration.

FROM the Report for 1883 of the Glasgow Museum we see that it had 223,129 visitors during the year. There were large additions to the Natural History Department during the year.

WE have already noticed M. Erkert's anthropological measurements in the Caucasus. He publishes now in the *Izvestia* of the Tiflis Geographical Society (vol. viii.) his further measurements and conclusions. The different nationalities appear as follows with regard to their cephalic indexes:—Only the Aderbaijan Tartars are mesocephalic (79·4), all others being brachycephalic, the indexes being 80·9 with the Kalmucks, 81·4 with the Ossets, 81·9 with the Adighe and Chechenes, 83·2 with the Little-Russians, 83·7 with the Georgians, and 85·6 with the Armenians. A high index was found for the Lezghines, but the number of measurements was only three. As to the height of the skull the Aderbaijan Tartars have the highest and longest heads; the Armenians the shortest and highest (71·1); the Kalmucks the longest but lowest (62·0); while the Little-Russians, the Adighe, and the Georgians afford intermediate types, the heights of their skulls varying from 67·6 to 66·7. All the above nationalities have relatively low and broad or chamäprosopous faces, there being, however, a number of individuals with long or leptoprosopous faces, especially among the Tartars. In connection with the above it may be worth noticing the measurements of M. Chantre of Lyons, published in the *Bulletin de la Société d'Anthropologie de Lyon* for 1883. It results from his measurements made on 137 Kurd men and 21 women, that their cephalic index is 81·4; they are thus brachycephalous, and sometimes mesaticephalous. The index increases with those Kurds who live close by Armenians, and decreases with those who live close by Bedouins. Altogether the memoir of M. Chantre ("Aperçu sur les caractères ethniques des Ansharies et des Kourdes") is an important addition to our knowledge of Kurdistan, as well as his second memoir, published in the same serial, on the Stone and Bronze Ages in Western Asia, Syria, Mesopotamia, Kurdistan, and the Caucasus.

IT appears from the Caucasian *Izvestia* that the Russian Amu-daria Expedition has arrived at the following conclusions:—The Ihan branch of the delta of the Amu could be easily made navigable; as to the possibility of bringing the water of the Amu to the Caspian, General Glukhovsky's Commission does not yet give a definite answer, but it considers it most probable. The immense and deep depression of Sary-kamysh could be turned by the canal; the necessary inclination of level exists; and the immense desert west of Khiva could be irrigated without difficulty and without loss to the oasis of Khiva.

IN the letter signed "O. S." last week (p. 525), under the heading "Remarkable Sunsets," the French term should be *pelure d'oignon* and not *velure.*

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mrs. F. Mortimer; two Secretary Vultures (*Serpentarius reptilivorus*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Blue-and-Yellow Macaw (*Ara ararauna*) from South America, presented by Mr. H. W. Kingdom; two Common Peafowls (*Pavo cristatus* ♂ ♀) from India, presented by Mr. R. F. J. Cobbett Allen; a Common Viper (*Vipera berus*, black variety) from Hampshire, presented by Lord Londesborough, F.Z.S.; a Yaguarundi Cat (*Felis yaguarundi*) from South America, a Leuhdorf's Deer (*Cervus leuhdorfi* ♂) from Amoorland, two Jardine's Parrots (*Pæocephalus gulielmi*) from West Africa, three Rhinoceros Hornbills (*Buceros rhinoceros* ♂ ♀ ♀) from the Malay Peninsula, two Nepal Hornbills (*Aceros nepalensis* ♂ ♂), a Green Cochoa (*Cochoa viridis*), two Nepal Tree Pies (*Dendrocitta nepalensis*), a Gray-headed Thrush (*Turdus castanea*) from Nepal, three Bronze Fruit Pigeons (*Carpophaga ænea*), two White-breasted Gallinules (*Gallinula phœnicura*) from India, two White-backed Pigeons (*Columba leuconota*) from the Himalayas, seven Waxwings (*Ampelis garrulus*), two Proteus (*Proteus anguinus*), European, purchased; a Lucian's Parrakeet (*Palæornis luciani*) from China, a Geoffroy's Dove (*Peristera geoffroii* ♂) from Brazil, received in exchange.

OUR ASTRONOMICAL COLUMN

COMET 1884 *a*.—The comet notified by telegram from Mr. Ellery as having been discovered in the constellation Grus, appears to have been detected by Mr. Ross, a young amateur astronomer residing at Elsterwick, near Melbourne, on January 7. Observations were commenced at Melbourne on January 12, and were continued to February 4, when the comet had become very faint. The positions, as first communicated to the *Astronomische Nachrichten*, contained more than one obvious error, and generally (according to a comparison made by Dr. Kreutz with an orbit since received from Melbourne) appear to be strangely inaccurate, a circumstance that will probably have caused useless expenditure of time to computers. We subjoin the Melbourne orbit with one calculated by Mr. Hind from the observations on January 12 and 28 and February 4, as they are printed in *Astron. Nach.*, No. 2579:—

	Melbourne		Hind
Perihelion passage, 1883, Dec. 25·7838 Melb. M.T. ... Dec. 25·4998 G.M.T.			
	° ′ ″		° ′
Longitude of perihelion	125 15 55	...	124 14·4
,, ,, ascending node	265 12 15	...	265 56·5
Inclination	64 53 16	...	64 59·7
Log. perihelion distance... ...	9·502384	...	9·51838

Motion—Retrograde.

It is to be remarked that Dr. Kreutz, calculating from the Melbourne orbit, does not reproduce the extreme positions stated to have been employed in its computation.

VARIABLE STARS.—On comparing the late Prof. Julius Schmidt's determinations of the times of minima of *Algol* in 1883 with the formula given by Prof. Schönfeld in his second catalogue of variable stars, it will be found that, by a mean of the observations between August 14 and December 4, the formula gives the minimum too late by fifty-eight minutes. The mean annual errors for the period 1876–83 have shown irregularity, but the separate results within the same year differ considerably.

Mr. Baxendell has worked out new elements for R Arietis from his own observations 1859–81. He finds for—

		Days
Maximum ...	Epoch 1866, Sept. 1·3 +	186·71 E.
Minimum ...	Epoch 1870, Jan. 2·3 +	186·63 E.

The mean interval from maximum to minimum is 99·0 days, and from minimum to maximum 87·7 days.

THE OBSERVATORY, CINCINNATI.—The seventh part of the publications of this Observatory has appeared. Parts 4, 5, and 6 were devoted by Mr. Ormond Stone to the double-star measures made with the 11-inch refractor in the years 1877–80. In the new part are given the observations of comets in the years 1880–82, including numerous physical observations as well as observations for position. There is a comparison with theory of the phenomena in the tail of the great comet of 1882. In a number of plates are illustrated the telescopic and naked-eye appearance of the great comets of 1881 and 1882 and of the first comet of the latter year.

Mr. H. C. Wilson is in temporary charge of the Cincinnati Observatory, Mr. Ormond Stone having been appointed Professor of Astronomy in the University of Virginia, and Director of the Leander McCormick Observatory.

THE "ASTRONOMISCHE GESELLSCHAFT."—The fourth part of the eighteenth volume of the *Transactions* of this Society is issued. It contains the proceedings at the meeting held in Vienna in September last and the usual critical notices of recent astronomical publications; also reports on the progress of the zone-observations from thirteen observatories. It was decided to hold the next meeting at Geneva in 1885; Prof. Auwers was chosen president for the second time, with Prof. Gyldén as vice-president, and Profs. Schönfeld and Seeliger (now at Munich) as secretaries.

PHYSICAL NOTES

THE transition-resistance supposed by Poggendorff to exist in electrolytic cells between the surface of the electrode and that of the electrolyte in contact with it has lately been investigated with great care by Prof. J. Gordon Macgregor in solutions of very pure zinc sulphate, using electrodes of amalgamated zinc. The conclusion arrived at was that such a transition-resistance, if it exists at all, is less than 0·0125 of an ohm.

IN another paper which appears in the *Transactions of the Royal Society of Canada* Prof. Macgregor describes an ingenious arrangement devised by him for measuring on Wheatstone's bridge the resistances of electrolytes. He employs alternate currents produced by a rotating commutator inserted in the circuit of two Daniell's cells; and in order to use with this arrangement an ordinary mirror-galvanometer, he recommutes the currents in the galvanometer circuit by means of a second commutator rotating on the same axle as the first.

THE annual *conversazioni* of the Société de Physique, of Paris, will be held this year on April 15 and 17 respectively, the former being limited strictly to the members of the Society. These meetings will, by the invitation of Admiral Mouchez, be held in the Observatoire.

A NOTE on Hall's effect was recently read at a meeting of the Physical Society of London by Prof. S. P. Thompson and Mr. C. C. Starling. They find that when a large sheet of foil is used, and placed symmetrically in a concentrated field between pointed magnetic poles, so that the junctions and connections are quite outside the influence of the field, Hall's effect is not produced. They find, however, an alteration in the equipotential lines of the current in the strip where it is magnetised, and have traced this effect to a change in the resistance. Strips of gold and tin show a decrease, strips of iron a slight increase of resistance when subjected to a strong magnetic field.

ANOTHER paper on Hall's effect appears in the current number of the *Journal de Physique* from the pen of M. Leduc. In this article M. Leduc draws a diagram of the equipotential lines, as, according to his ideas, they will be found to lie between the two "parasitical" electrodes. It does not appear whether he has verified his views by actual determinations of the position of the lines of equal potential.

ROWLAND'S famous experiment demonstrating the magnetic action of electric convection has been called in question by Dr. E. Lecher of Vienna. In Rowland's original experiment the electrified rotating disk was horizontal, and the magnetic needle, protected from electrostatic influences by being inclosed in a metallic case, was held over the disk at a point near the circumference. Dr. Lecher, in attempting to repeat the experiment, placed the rotating disk in a vertical plane, its axis being horizontal; the magnet needle was placed parallel to the plane of the disk and in the axis of its rotation in fact relatively as the coil and needle of a Gaugain galvanometer. Disks of brass and of *papier-maché* covered with graphite were used, and charged

from a Holtz machine to potentials of about 5000 volts as measured on an absolute electrometer. The velocity of rotation was about 200 revolutions per second. The astatised needle was protected within a metal case, and was observed in the usual way by a mirror. No deflection was observed either when the disk was still or when it rotated. Dr. Lecher intends to repeat Rowland's experiment with the original horizontal disposition of the disk.

DR. LECHER has also made another experiment of great interest. A ray of light was divided, as in many experiments on interference, into two parts, which, after passing through two parallel glass troughs, were caused to reunite, giving the usual interference-bands. The troughs contained strong solutions of nitrate of silver. By means of electrodes of silver an electric current of 6 amperes strength was carried in opposite directions along the troughs so that in one trough the current flowed *with* the light, and in the other against it. But in no case was any displacement of the fringes observed. Dr. Lecher concludes that the velocity of light is not influenced by a current flowing through the medium.

DR. LECHER has made a third and still more interesting experiment, attended, however, like the preceding, with a negative result. This was an attempt to prove whether Faraday's famous experiment of rotating the plane of polarisation by an electric current could be inverted. He has attempted to generate currents by rotating the plane of polarisation of light. The arrangement was as follows:—A ray of plane-polarised light was sent through the interior of two powerful helices of wire situated at some distance from one another. Through the first of these a powerful alternate current was sent, which impressed upon the ray a rapid oscillation of its plane of polarisation. The second helix was connected to a sensitive receiving telephone in the hope that sounds might therein be heard, as would be the case if the rapid rotations in the plane of polarisation of the ray were capable of setting up currents in the surrounding wire. Absolutely nothing was, however, heard.

BACTERIA

A VERY distinguished audience assembled at the Parkes Museum on Thursday evening, March 27, to witness Mr. Watson Cheyne's demonstration of pathogenic micro-organisms. The chair was taken by Sir Joseph Lister, Bart. After stating that the great group commonly called Bacteria might most conveniently be subdivided into four classes—(1) Micrococci (round bodies), (2) Bacteria (small oval or rod-shaped bodies), (3) Bacilli (large rod-shaped bodies), and (4) Spirochætæ and Spirilla (rods spirally twisted), and dwelling on the great variety as well as importance of the various parts played by this great group in the economy of nature, Mr. Watson Cheyne demonstrated numerous micro-photographs taken by Dr. Robert Koch, as well as some drawings by means of a limelight apparatus. He observed that great differences existed among the various bacteria in their behaviour towards the human body: some could be injected without causing any injury, others could not grow in the living body, but could develop in dead portions of tissue and the secretions of wounds, giving rise to poisonous products. The true pathogenic organisms were able to attack the living body and multiply in it; they included the organisms which found entrance through some wound, giving rise to the traumatic infective diseases, and others which could obtain entrance without observable wound. Further, certain organisms, such as the *B. anthracis*, were capable of growing outside the body in dead organic substance, while others, such as the *B. tuberculosis*, were apparently only capable of development in the living organism or under artificial conditions which reproduced to some degree those existing in the tissues of warm-blooded animals, though capable of long retaining their vitality in the dry state. With regard to the traumatic infective diseases, he thought that the most absolute proof had been furnished that the bacteria found in them, and nothing else, were the causes of these diseases. To establish such a proposition it was necessary that an organism of a definite form and with definite characteristics should always be found in the blood or in the affected part. The blood or the affected part when inoculated into another animal of the same species must produce the same disease. When the blood or the affected part was inoculated on a suitable soil outside the body, the micro-organisms grew, and must be indefinitely propagated on similar soil. When in this manner the organisms had been separated from the remains of the materials in which they were embedded, their inoculation in an animal must produce again the same disease, the same organisms being found in the diseased parts. These conditions had now been fulfilled with regard to anthrax, septicæmia of the mouse, erysipelas, tuberculosis, glanders, and acute pneumonia. With regard to typhoid fever, relapsing fever, cholera, and ague, the evidence was very strong, but not conclusive. Mr. Watson Cheyne concluded by dwelling on the importance of surrounding circumstances, chiefly those summed up in the phrase unhygienic conditions, as concomitant causes of disease by preparing the blood for the attacks of these micro-organisms.

The chairman, Sir Joseph Lister, dwelt upon the important fact that the organisms which produced particular diseases were only able to develop under very special conditions, instancing the bacillus which caused septicæmia in the house mouse, but which was unable to produce any deleterious effect on the field mouse. He thought this fact, which showed that the very slight difference in the blood of these two animals was sufficient to alter the conditions favourable to the development of the bacteria, might prove of very great interest, as it was possible to conceive that by the administration of some medicines, sufficient alteration might be produced in the blood of the human system to kill off or to prevent the development of any special bacteria on the first appearance of the symptoms of the disease in the patient. Sir Joseph Lister concluded by referring at some length to the importance of Pasteur's researches on modified virus.

Prof. Humphry paid an eloquent tribute to the great work which Sir Joseph Lister had already achieved, and looked forward with a large hope to the future of medicine.

THE STABILITY OF SHIPS

PROFESSOR ELGAR has recently made two important contributions to this important question; the first was read before the Royal Society on March 13 last. The main object of the paper was to exhibit the manner in which the stability of a ship varies with changes of load and draught of water such as merchant steamers are liable to. None of the properties possessed by a ship is more vital to her safety and efficiency than that of stability. At the same time none is dependent for its existence and amount upon so many or such diverse and variable circumstances as it. The stability of a ship, both as regards moment and range, is affected not only by the position of her centre of gravity, which largely depends upon stowage, but also by draught of water. If the centre of gravity be kept fixed in position at various draughts of water, the stability will still vary very considerably with the draught, and often in a manner that contains elements of danger.

The usual practice in investigating a ship's stability is to calculate a curve of metacentres, and one or more curves of stability at certain fixed draughts of water and with given positions of centre of gravity. The curve of metacentres gives the height at all draughts of water above which the centre of gravity cannot be raised without making the ship unstable when upright, and causing her to lie over more or less to one side. The ordinates of the curve of stability represent the lengths of the righting arms, which, multiplied by the weight of the ship, give the righting moments at all angles of inclination from the upright. The stability of numerous vessels, both of the Royal Navy and mercantile marine, have been investigated in this manner for certain draughts of water, and a great amount of information obtained respecting the variation of stability with inclination at such draughts, and the angle at which the stability vanishes in many classes of ships. The peculiar dangers attaching to low freeboard, especially when associated with a high centre of gravity, have been fully discussed and made known.

Curves of stability have been chiefly constructed for deep and moderate draughts; the character of the stability which is often to be found associated with very light draught, appears to have hitherto escaped attention. As a matter of fact, light draught is often as unfavourable to stability as low freeboard, and in some cases more so. The general opinions that have till recently prevailed upon the subject appear to have been based upon a vague impression that so long as a vessel has a high side out of water, and any metacentric height, she will have great righting moments at large angles of inclination and a large range of stability. It was shown at the *Daphne* inquiry, held by Sir E. J. Reed in

July last, that these opinions largely prevailed and were erroneous.

Prof. Elgar was called upon to make some investigations respecting the stability possessed by the *Daphne* at the time of the disaster which happened to her, and to give evidence respecting the same. He afterwards pointed out, in a letter to the *Times* of September 1 last, some of the considerations which obviously apply to light draught stability. The first, which it appears had never before been stated, is that any homogeneous floating body which is symmetrical about the three principal axes at the centre of gravity—such as a rectangular prism or an ellipsoid—will have the same moment of stability at equal angles of inclination, whether floating at a light draught with a small volume below water, or at a deep draught with a similar volume above water. For instance, if a homogeneous prism of symmetrical cross-section 5 feet high float at a draught of 1 foot, it will then have precisely the same moment of stability at equal angles of inclination, and consequently the same curve of stability throughout, as if it were loaded—without altering the position of the centre of gravity—till it had 4 feet draught of water, and 1 foot of free board. From this it follows that, in such elementary forms of floating bodies, lightness of draught has the same effect upon stability as lowness of freeboard; and if a low freeboard is unfavourable to stability, so also, and precisely to the same extent, is a correspondingly light draught of water. This proposition can be made still more general, as it applies to homogeneous bodies of any form of cross-section which revolve about an horizontal axis fixed only in direction. From this may be deduced the results given by Atwood in his papers read before the Royal Society in 1796 and 1798 respecting the positions of equilibrium and other peculiarities connected with the stability of floating bodies.

In considering the stability of a ship at various draughts of water, and comparing it with that of the class of figures above described, modifications require to be made for the departure from symmetry of form, and for the extent to which the vertical position of the centre of gravity differs from what it would be if the external surface inclosed a homogeneous volume. Prof. Elgar has done this with great fullness of detail in his paper, and shows, by means of curves, how the stability varies with draught of water at constant angles of inclination in various geometrical forms of floating bodies, and in a large passenger steamer of ordinary type. The curves thus dealt with are curves of righting moments, and not merely curves of lengths of righting arm. The ordinary curve of stability is usually made for lengths of righting arm, because the displacement is constant, and the same curve therefore gives upon different scales, either lengths of righting arm or righting moments. In the vertical or cross curves of stability, however, such as are now being dealt with, draught, and therefore displacement, is one of the variable quantities, and curves of righting moments are of a very different character from curves of righting arm. Complete cross curves for a ship, from which ordinary curves of stability can immediately be obtained for any draught of water and position of centre of gravity, can be constructed in a few days with the aid of Amsler's mechanical integrator.

Prof. Elgar shows conclusively the necessity in many cases of regarding the stability of a ship from the point of view of variation of righting moment with draught of water, the angle of inclination being constant, instead of from that of variation of righting moment with angle of inclination, the draught being constant, as is usually done; or rather of considering the subject from both points of view instead of almost exclusively from the latter. He also shows that it is necessary to investigate, more fully than has formerly been done, the moments and range of stability of ships and other structures that may be intended to float at very light draughts of water.

Prof. Elgar's second paper was read last week at the meeting of the Institute of Naval Architects; its title was "The Use of Stability Calculations in Regulating the Loading of Steamers."

The stability of ships, Prof. Elgar went on to say, is a subject that has received a considerable amount of theoretical investigation during recent years. The general character of the stability of certain classes of ships, and the circumstances which affect it, have been largely ascertained and made known; while the methods of performing the requisite calculations—especially when large angles of inclination are being dealt with—have been greatly improved. Curves of stability have been constructed and made public for numerous ships of various classes, both for war and mercantile purposes.

The results of the investigations that have thus been made are of great value to naval architects and men of science, and enable them to know much more respecting the actual stability often possessed by ships than was possible with the imperfect data available in former years. In the case of ships that are built for purely war and some other special purposes, the ordinary stability calculations enable instructions to be readily framed respecting the stability they possess under ordinary working conditions, or in such critical circumstances as may possibly occur during their career. Any risks of instability that may exist, or arise in certain contingencies, may be ascertained, and the precautionary measures necessary for counteracting them devised and pointed out.

The problem that has to be dealt with in advising those in charge of war ships how to effectually guard against instability, is well within the grasp of the naval architect. In such vessels the loading is mainly of a permanent character, while that part of it which is subject to variation, such as coals, stores, ammunition, &c., varies in a manner which can be readily taken into account in the calculations. Curves of stability that are constructed for war ships for three leading conditions, viz. (1) the fully-laden condition; (2) the same, but with all the coals consumed; and (3) the light condition with all coals, ammunition, and consumable stores expended, are usually sufficient to enable full instructions to be framed for the prevention of instability. In some war ships there are other critical conditions which may require consideration, such as the possible injury and laying open to the sea, of compartments not protected by armour; but in all these cases the conditions are comparatively fixed, and may be allowed for in the calculations. When curves of stability have once been constructed for a war ship to represent the various critical conditions to which she may be subjected, they are always applicable, and may be relied upon to furnish, at any time, a safe guide to her stability.

In the case of mercantile steamers, however, except such as carry no appreciable weight of cargo, the problem of how to apply the results of stability calculations to the guidance of those who have to work and stow them is of an entirely different character. The naval architect cannot control, or even estimate, the amounts and positions of centre of gravity of the various items of weight that make up the loading to anything like the same degree of certainty as in war ships. There are many steamers afloat in which the cargo is nearly or quite twice the total weight of the vessel, together with her machinery and equipment. In such cases the naval architect can only control in the design about one-third of the total weight of the vessel and her cargo, leaving the remaining two-thirds in the hands of the owner, master, or stevedore. It is obvious, therefore, that whatever may be the qualities of the empty vessel in respect of stability, these may be greatly modified or entirely altered by the manner in which she is loaded. It is the loading to which we must look in the large proportion of cargo-carrying steamers for the due preservation of such stability as is necessary for safety at sea.

It is in this direction also that we have to look for the cause of a great many of those losses which have occurred at sea during recent years, and to which attention was first prominently called by Mr. B. Martell, the Chief Surveyor of Lloyd's Register Society, in a paper read before this Institution in 1880 upon the causes of unseaworthiness in merchant steamers. Mr. Martell attributed, and quite rightly so, a great many of the losses of steamers to instability; and there can be no doubt that this cause of loss still continues to operate very largely. The evidence given at Board of Trade inquiries in cases of missing steamers is constantly pointing to instability as the cause of loss, although the full meaning and weight of the evidence may not always be fully and accurately appreciated at these inquiries. It often diverts attention from the main cause of loss to say that it occurred because the ship was unstable. The fact is, that the ship has frequently so little to do with the matter, and the stowage so much, that it is the latter which should be blamed for the instability, and not the ship herself. When a ship is built for a particular trade and for the purpose of carrying certain specific cargoes she may then, of course, be so designed as to be quite stable, in all conditions, while thus employed; but when vessels are built, as they often are, to dimensions fixed by owners, for general trading purposes, it is seldom possible for the designer to provide against instability arising in some possible or con-

ceivable circumstances of loading. The due preservation of stability in such cases requires to be watched and provided for by those who control the loading.

It is erroneous to suppose, as appears to be sometimes done, that a cargo-carrying steamer should be so constructed and proportioned as to run no risk of becoming unstable, however she may be laden. If this idea were acted upon, such a mode of preventing instability, however easy and plausible it may at first sight appear to be, would only defeat the desired object of promoting safety at sea, because it would make many vessels dangerously stiff when laden with some classes of cargo. The true and reasonable mode of procedure is not to attempt to construct a ship so that she will be stable however she may be laden, but to see that any tendency she may have towards instability—if any such exist—is understood by those in charge of her, and that she is always laden with careful reference to it. There are no steamers afloat, whatever tendency they may have towards instability as sometimes laden, that they may not be kept perfectly safe if treated with full knowledge of what their stability is, and the stowage regulated accordingly. One great problem that the mercantile naval architect has just now to solve is, how any dangerous features of a ship's stability are to be made clearly known to those in charge of her, and in what manner they can be best taught to regulate the loading in cases where special care may be required.

It is sometimes supposed that owners and masters are not only negligent, but indifferent in this matter; and that they deliberately refrain from any consideration of it. It has been stated that there are no owners who avail themselves of the knowledge of stability now readily obtainable as a guide in the stowage and safe working of their ships. These are views which my experience does not enable me to indorse. I have found, on the contrary, that many of our leading owners of passenger and cargo steamers are extremely anxious about the matter; and not only anxious, but they adopt all means that lie within their power of dealing practically with it. The great stumbling-block they usually meet with, however, is the intrinsic difficulty of the subject.

Owners and masters have their own modes of thought and their own practical methods of ascertaining and regulating the stability of their ships, which are often quite sufficient for the purpose. They can very well comprehend whether a vessel will stand up when light without ballast, and, if not, how much it will require to make her do so. They can also understand if she is too stiff when laden with heavy dead-weight cargoes placed low down in the hold: or if she becomes unduly tender when laden with light cargoes of which more than a certain quantity is placed in the 'tween decks. They have not, however, had the technical training and experience which is requisite to enable them to understand and deal with metacentres, centres of gravity, and curves of stability; and to make all those allowances for constant variations in draught of water and position of centre of gravity which the different cargoes they carry render necessary. Some owners have recently obtained curves of metacentres and curves of stability for their ships, constructed for certain draughts of water and descriptions of cargo. These curves, as a rule, are put to no real practical use by them, as they find themselves unable to apply stability information in this highly specialised form to the accurate and reliable treatment of the various questions that arise in loading, or to compare it with the results of their own judgment and experience.

The above course has lately been taken in many cases because of the opinions which have been expressed that the way to prevent ships being lost through want of stability is to supply the masters with particulars of the metacentric height and a curve or curves of stability. The Wreck Commissioner advocates this course, and appears to entertain no doubt as to its desirability and practical efficacy. His object is a most praiseworthy one, but I do not believe it to be possible to carry it out in the way he suggests. The advice he gives is based upon the belief that shipmasters and others who have to do with the loading of ships can readily be made to understand what curves of stability represent, and to use them correctly in practice. I have during the last two or three years frequently tried to carry out this view, but have never yet met with a shipmaster—and I have had to do with some of the most capable and intelligent of the class—who could be got to understand curves of stability sufficiently well to be trusted to work with them in practice, or who would even profess that he could do so.

If mercantile steamers could always be loaded in a uniform manner, it might be possible to represent their stability in all conditions with sufficient accuracy and completeness for all working requirements by means of a curve or curves; but as regards the vast bulk of merchant shipping there are no curves of stability which could possibly be constructed, except that for the absolutely light condition, which would be likely to represent the actual stability of the ship except on a very few occasions during the whole of her career. The only use to which any curves of stability that might be furnished could, as a rule, be put is to furnish data for enabling the stability under different conditions from those for which they were constructed to be estimated. This is an operation which masters of ships cannot perform, and which would only be likely to confuse and mislead them if they were to attempt it.

The Wreck Commissioner laid great stress upon the use it would have been to the captain of the *Austral* at the time of the accident if he had been in possession of curves of stability and calculations which had been constructed for that condition, and laid before the Court. It does not appear to have been seen that, whatever particulars of calculations and curves of stability had been supplied to the captain, he could not by any possibility have had those which related to the condition of the ship at or somewhat prior to the time in question. Her stability on that occasion was determined by the amount of weight she happened to have in her, and the position of its centre of gravity; and this was the result of a chance state of things which only existed at that precise moment, and which may hardly occur again during the existence of the ship. If we assume that this information would have taught the captain more about her stiffness than he already knew through his previous experience of the vessel, still it could not have been supplied to him beforehand by any one. All that could have been done was to supply him with particulars of the stability at other draughts and with other positions of the centre of gravity, leaving it to him to estimate from these what it would be at the time in question if he thought it desirable to do so.

I need hardly say again that the operation of constructing curves of stability for a particular draught of water, and position of centre of gravity from the results of calculations made in the usual way for certain other draughts of water and positions of centre of gravity, is an operation which requires a well-trained naval architect to perform. No one knowing the subject can suppose that masters of vessels have had either the training or the experience to qualify them for performing such an operation, or can help fearing that the result of their attempting it might be misleading. As I have already said, I have never been able to discover a shipmaster who could be safely trusted to do it, or who cared for it to be supposed that he could. It is hopeless, at present, to expect either shipowners or shipmasters to use metacentric heights and curves of stability as a practical guide in stowage; and it is necessary to put stability information before them in a simpler form, and one which fits in better with their own ideas and modes of procedure, if it is to be utilised in furnishing any real guide towards safety in loading. It is quite unnecessary for us to require such persons to become specialists in the science of naval architecture before applying the results of scientific calculations to safeguarding the stability of their ships. I have myself been obliged to give up all attempts to deal satisfactorily with the question by supplying curves of stability and other information of that class.

The method which I have adopted is the following, and I now lay it before the Institution, chiefly for the purpose of eliciting opinions upon the subject, and as a suggestion to others who may be working in the same direction and have experienced similar difficulties with myself. In advising upon how a steamer should be treated and loaded so as to be kept safe in respect of stability, I state (1) the quantity of ballast, if any, that is required to enable her to stand up when quite empty, without water in boilers or tanks, coal in bunkers, and with a clean-swept hold, and to be stiff enough for all working requirements in dock or river; (2) if she is to be employed in carrying homogeneous cargoes, what proportion of the space in the 'tween decks it is safe to fill with such cargo, after the holds are full, and what weight of ballast is required in the bottom to enable the vessel to be loaded to her maximum draught with such cargo; (3) if required to carry two or more kinds of homogeneous cargo, such as grain and cotton, grain and wool, grain, meat, and wool, &c., the best mode of stowage, and whether or not the space in the 'tween decks can be filled with the lightest of the cargoes, and in what circumstances ballast, and how much of it, will be required; (4) if not intended for homogeneous cargoes, but for general cargoes, or partly homogeneous and partly general, the

average densities of the general goods for various ports is arrived at after a little experience, and the same system adopted. The main point is, to state what space, if any, must be left unfilled in the 'tween deck cargo spaces, with the different descriptions of cargo, and what ballast, if any, is necessary if the vessel is to be loaded to her maximum draught; (5) if the consumption of the coal diminishes the stability materially, as is often the case in some classes of steamers, to call prominent attention to this fact, in order that the captain may not be misled by finding his ship appear to be rather stiff on commencing a voyage. The possible consumption of coal is, of course, taken into account in fixing upon the limits that should be imposed upon the stowage in all the conditions named; and (6) if there appear to be any circumstances in which a tendency towards instability may arise they are described, and suitable precautions suggested. I believe that Lloyd's Register Society, in fixing a load-line for vessels that may in some conditions be laden so as to have insufficient stability, describe the stowage that is requisite for safety in somewhat similar terms to the above.

General particulars, such as these, respecting the character of a ship's stability in different conditions, may be made to convey all the information that is necessary for the effective prevention of instability, and I find that they are appreciated by owners and masters, and actually used as a guide in the loading of ships. They may be made to fully define all the essential points upon which stability depends, and are expressed in a form and language that is understood by those who have to use them. This is shown by the fact that telegrams are sometimes received from foreign ports respecting ships which are to be laden with cargoes somewhat different from those to which the specific instructions apply, describing the cargoes that are to be carried, and asking whether any different arrangement of ballast or proportion of weight in the 'tween decks from what has been prescribed for some other cargo is necessary. Such inquiries show that intelligent use is being made of the information supplied, and that it is being utilised for practical guidance in loading.

One of the main reasons why it is better to give information in this simple form is that it obviously fits in with a shipmaster's own practical modes of thought and ideas respecting stability. It is a mistake to suppose, because owners and masters cannot express their views respecting the stability of ships in scientific language, that they therefore have no views that are worth anything. The fact is, that the masters of ships very often have quite correct ideas respecting the stability of their vessels and how to load them. If they see a vessel quite empty in dock, and observe the effect of moving weights in and out when light, they often acquire as much knowledge of her stability in the light condition as is requisite for all purposes of safety and efficient working. They also, by means of experience obtained in loading, frequently get to know as much about the stability of certain classes of vessels in the laden condition as is necessary for practical purposes, and certainly for all purposes of safety. Whether sufficient knowledge can be gained in this way or not for all possible requirements depends largely upon the type and peculiarities of a vessel. As a rule, it is all that is applied to the purpose, and there can be no doubt that in many cases it may be sufficient. It is in vessels which contain elements of danger that cannot be discovered in this practical manner that a different and more scientific mode of treatment becomes requisite.

The proper use of stability calculations is not to supersede or interfere with that knowledge of a vessel's qualities which may be gained by experience but to supplement and complete it in certain cases where it may be necessary. As an illustration I may refer to the small range of stability sometimes found to be possessed by deep vessels of low freeboard. The discovery of the dangers to which such ships are liable may perhaps be successfully made in some instances by simply observing their behaviour at sea; but probably it is more often made only when the ship capsizes. Then, again, many ships become unstable at sea through the consumption of their bunker coal, particularly when a large portion of such coal is carried, as it sometimes is, in a reserve bunker under the lower deck. There are cases in which the metacentric heights of cargo-carrying steamers are reduced by $1\frac{1}{2}$ feet by the mere consumption of the bunker coal. In such cases instability may very readily arise at sea in a manner of which the captain is unable to form any accurate conception when merely judging by the results of his own experience. This is particularly likely to be the case when alterations are made in bunkers, or when portions of the hold are added as reserve bunkers for enabling voyages of longer duration to be made than have previously been contemplated. I certainly believe, as the result of an examination of the stability of many mercantile steamers, that a great number of vessels are lost at sea from each of these causes, viz. through capsizing on account of low freeboard and consequent small range of stability, and also through loss of stability by reason of the consumption of coal. In both of these classes of cases the danger is aggravated if the ships are flush-decked, without any or with but small water-tight erections above the upper deck.

It is very difficult to make a complete analysis of the various causes of loss at sea, and to show conclusively what is the relative mortality of vessels of various types and different descriptions of cargo. The difficulty is due to the fact that the Board of Trade returns are not compiled in a manner which enables all the necessary information to be extracted from them. So far as it is possible to judge, however, by the particulars available, it appears that the types of steamers that are least subject to mysterious losses at sea are those which have long ranges of water-tight erections on deck, and are therefore least liable to become unstable. I believe that the comparative immunity against loss which appears to be possessed by many efficiently built and protected "well-deck" steamers, is largely due not only to their comparatively low centre of gravity of cargo, but to the righting power furnished at large angles of inclination by their extensive deck erections. This is undoubtedly the case, notwithstanding the fact that seas may break into the well, and often fill it with water. It may be somewhat startling to persons familiar with the loading of flush-decked steamers, to find many well-decked vessels making voyages across the Atlantic with portions of their decks so near to the water as they sometimes carry them; but a little examination suffices to show that the fact of the water entering a properly constructed and fitted and moderately sized well cannot do much to endanger the safety of the ship. Any effect it may have upon the stability is only at small angles of inclination.

In order to show how small is the effect of water in the well of an ordinary first-class steamer of this type upon her stability, I have given two curves of stability in Fig. 1 for such a vessel. That marked P is for the condition of no water being in the well till the vessel is inclined sufficiently for the edge of the deck to become immersed, and that marked Q for the condition of the well being filled with water before the inclination commences. Mr. Martell was good enough to have these curves calculated for me, in order that I might have them in time for the reading of this paper. They are for a raised quarter-deck vessel 257 feet by 35 feet 6 inches by 18 feet 6 inches, with a well 60 feet in length, and bulwarks over 5 feet high; the freeboard amidships to the main deck being 2 feet 2 inches. Prior to the water entering the well the vessel is assumed to be at her usual trim of about a foot by the stern, and a correction is made for the change of trim caused by the filling of the well. No allowance is made for the quantity of water that would be thrown out of the well by the movements of the ship, but it is assumed to be possible to completely fill it with water to the height of the rail at the fore end of the bridge, and for no other way of escape to exist for the water but that of pouring over the rail as the vessel inclines. The freeing ports and scuppers are not assumed to have any effect in clearing the deck of water. The weight of water which the well will hold when the vessel is upright is 186 tons, but when she is inclined to 10° it will only hold 98 tons, and when inclined to 20° it becomes reduced to 28 tons. These figures and the curves in Fig. 1 show that water in the well of such a vessel cannot materially affect her stability after a small angle of inclination has been reached, and that so far as stability is concerned the well cannot be regarded as a serious element of danger.

A practical point of great importance in determining the amount of stability a ship should possess at sea is the minimum metacentric height that may be regarded as sufficient for safety. Different types of vessels have quite different characteristics in respect of stability. War ships, and some classes of merchant steamers, require large metacentric heights in order to insure sufficient righting moments at moderate angles of inclination, and a safe range of stability. The curves of stability given in Fig. 2 apply to such a case. Those curves belong to a typical three-decked steamer, without any water-tight deck erections, 280 feet by 34 feet 6 inches, by 24 feet 6 inches. The mean load draught is 22 feet 6 inches, and displacement 4400 tons; the freeboard being 5 feet 4 inches. The metacentric

height is 6 inches for the curve A, and 1 foot for the curve B. It is obvious that, in judging of the safety of small metacentric height for such a vessel, the range of stability is an important factor to be considered. The range necessary for seaworthiness largely determines and often fixes the limit below which the metacentric height should not be reduced in such a type of vessel and in many others.

But there are very large numbers of steamers, such as passenger liners and cargo steamers, of the spar and awning-deck classes, which generally have very large ranges of stability, and large righting moments at great angles of inclination, whatever the metacentric height may be; and in many cases, even with no metacentric height at all. In such cases the minimum metacentric height which is essential to safety and efficiency has to be determined by entirely different considerations from those which apply to war ships, and those classes of mercantile steamers whose stability is of the character shown by the curves in Fig. 2. When we have to deal with vessels which even with no metacentric height will return to the upright, provided water does not get into the ship, and no large weights shift, whatever angle of inclination may be reached, the conditions of the problem are entirely changed. The principal object which then has to be

Fig. 1.

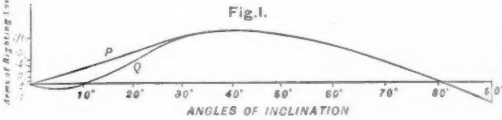


Fig. 2.

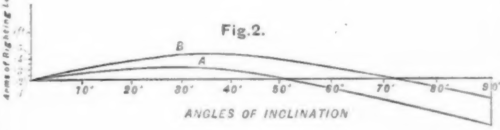


Fig. 3.

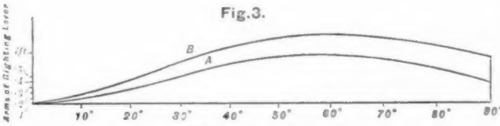
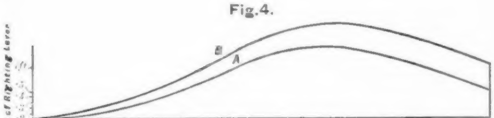


Fig. 4.



considered is to prevent too easy an inclination from the upright by the action of the wind and other forces which may operate upon her; and the question mainly turns upon what may fairly be considered sufficient for this purpose.

Many persons have been surprised on first learning how little metacentric height many high-sided mercantile steamers are in the habit of working with in safety. There are many steamers of the spar and awning-deck classes employed in carrying homogeneous cargoes, which have been performing their work for years, not only with perfect safety but without showing any signs of what nautical men call tenderness, the metacentric heights of which, during certain periods of their voyages, are frequently not more than 8 inches or even 6 inches. The latter figure may probably be regarded as about the minimum which such vessels approach without indicating to those on board that they are becoming unduly tender: but it is quite certain that many never show any such signs, and appear to be perfectly safe with 8 inches of metacentric height.

Vessels of this class have curves of stability of which those shown in Figs. 3 and 4 are types. The curves in Fig. 3 are for a spar-decked steamer 318 feet by 40 feet by 22 feet. The load draught is 23 feet 6 inches, and displacement 5760 tons; the freeboard being 8 feet 6 inches. Those in Fig. 4 are also for a spar-decked steamer 220 feet by 30 feet by 23 feet. The load draught is 16 feet, and displacement 2000 tons; the freeboard being 8 feet 6 inches. The curves marked A in each of these figures are constructed for 6 inches of metacentric height, and those marked B for 1 foot, in order that they may be compared with the corresponding curves in Fig. 2. The metacentric height of 6 inches is about what each of these vessels would have if laden to the draughts named with homogeneous cargoes, such as they frequently carry; and the metacentric heights of 1 foot are obtained by leaving a portion of such cargo out of the 'tween decks, and replacing it by an equal weight of ballast in the bottom.

It will be seen that the increase of righting moment in Figs. 3 and 4 continues up to a very large angle of inclination. This increase of righting moment tends to prevent dangerous inclinations being reached, while the smallness of the metacentric height causes such vessels to be very easy and comfortable in a seaway. Some steamers whose stability is of this character are vessels which carry cargoes liable to shift, such as grain or coals, and it may be thought that with cargoes of this class a small metacentric height is particularly unsafe, and that considerable initial stiffness is necessary to prevent any danger arising through shifting of cargo. Any opinions that may be formed upon this point are necessarily more or less speculative, as we have but little exact information to go by; but it should be borne in mind, in considering the question of initial stiffness in connection with shifting cargoes, that, although such stiffness increases the resistance to inclination, it increases at the same time the tendency to roll, and to displace or shift the cargo.

The question of the minimum metacentric height which may be regarded as consistent with safety in those types of ships where it is not governed in any degree by the necessity of providing range of stability, as shown by Figs. 3 and 4, is a subject which has never been much discussed, and which, on account of its important and immediate bearing upon the safety of many vessels at sea, is, in my opinion, deserving of the consideration of this Institution. If any of the remarks contained in this paper should serve to elicit opinions, information, or facts bearing upon the subject, my purpose in making them will be answered.

I may add, in conclusion, that the following are the main points which I have desired to lay before the Institution in this paper:—(1) The form in which the results of stability calculations can be put before owners and masters of mercantile steamers, so as to be of the greatest practical use in loading such steamers, and regulating their stability in accordance with the requirements that may arise; (2) the fundamental difference which exists between the relation of righting moments at large angles of inclination and range of stability to metacentric height in the various types of steamers, as shown by Figs. 2, 3, and 4, such relation making it necessary to fix the minimum metacentric height that should be allowed with due regard to the righting moments at large angles of inclination in some cases and unnecessary to do so in others; and (3) the minimum metacentric height that may be regarded as consistent with safety in cases where range of stability and the righting moments at large angles of inclination are so ample as not to call for consideration. The two latter points are so intimately connected with the first that they naturally require to be considered along with it.

THE INSTITUTION OF NAVAL ARCHITECTS

THE Institution of Naval Architects held its twenty-fifth Session at the Rooms of the Society of Arts on April 2, 3, and 4, Lord Ravensworth in the chair. Whilst the papers read were of course mainly on technical questions of naval construction, equipment, &c., some of them possessed points of general scientific interest, of which a brief account may be given. The President's address dealt mainly with what may be called the economic side of the shipping industry, dwelling on such points as the Merchant Shipping Bill, the length of time occupied in building ships of war, the depression of the carrying trade, &c. Passing on to the papers contributed, the first read was by Mr. J. D. Samuda on the *Riachuelo*, a steel armour-clad twin screw turret-ship of 6000 tons displacement, and 6000 horse-power, lately built by his firm for the Brazilian Government.

The second paper, by Mr. A. F. Yarrow, was on an Electrical Launch tried last year both on the Thames and on the Danube,

and already mentioned in these columns. It dealt with the question from a practical and not an electrical point of view, and is so far valuable as presenting a fresh aspect of the question. On the whole the author's conclusions are satisfactory. He considers that there is even now a field for electrical launches in cases where the conditions are favourable, such as having a supply of cheap motive power for recharging the batteries; and that they are pre-eminently adapted for torpedo boats, owing to their being always ready for action, and their complete noiselessness when in motion. On the whole the advantages and disadvantages as compared with the steam-launch are summed up by Mr. Yarrow as follows, beginning with the former:—

1. Entire absence of noise.
2. Great cleanliness.
3. The whole of the boat is available for passenger accommodation, the midship or best part of it not being occupied by machinery.
4. When once charged it is ready for use at a moment's notice.

The points against it are:—

1. Difficulty and delay from frequent charging.
2. Greater first cost.
3. Greater cost of working in those cases where an engine has specially to be laid down for the purpose of charging.

The third paper read was on the Vibration of Steam-vessels, by Mr. Otto Schlick, which dealt with the shaking so well known to passengers on screw steamers from the practical and theoretical point of view. It is shown clearly that the phenomenon is merely due to the fact that the ship, considered as an iron girder, has one or more fixed periods of vibration depending on her length, her width, and other dimensions. With regard to the practical means of overcoming such vibrations, it is pointed out that anything which causes the engine to run at a different speed, for instance, the putting in of a new propeller, will probably have a favourable effect. The shifting of the screw to a different angle with regard to the cranks is recommended as often giving a good result, inasmuch as two of the forces causing the vibration may be balanced one against the other. An ingenious apparatus for measuring such vibrations is described by the author.

The morning of April 3 was occupied during the whole period of five hours by the reading and discussion of three papers on the burning question of Stability. One of these, on the Use of Stability Calculations in Regulating the Loading of Steamers, by Prof. Elgar, we print at length. Another, on Cross Curves of Stability, was read by Mr. W. Denny, the well-known shipbuilder of Dumbarton. He observes that stability curves are required for at least four draughts of any steamer, viz. the launching condition, the condition completely finished, but without any cargo, coals, &c., on board, the fully loaded condition, and the condition with the coals consumed. If the stability curve be also calculated for an intermediate draught between the second and third of these, five points will be obtained at each angle, by means of which a cross curve of stability can be produced. It is therefore of great importance to work out such cross curves and to find a method by which they can be readily constructed from the ordinary curves of stability. A method for doing this with the assistance of Amsler's Integrator has been devised, and when drawn the curves are also represented by means of a solid model. These cross curves are each for a given angle, and have the length of the righting arm varying with the draught or displacement. With such cross curves in number sufficient to cover angles at intervals of 10°, 15°, and 20°, and each ranging through all the draughts from the launched to the loaded condition, ordinary curves at any draught and with any height of centre of gravity can be easily obtained, and with great rapidity. The method employed is fully described, as is also another method due to Mr. Couwenberg. Tables are also given showing the results obtained for the same steamer by the two methods, which, though worked out separately, were found to agree very closely.

The third paper was on a New Method for Calculating the Stability of Ships, by M. Daymond. This is an elaborate paper of a theoretical character, illustrated with numerous diagrams. It gives the history of the means adopted for calculating stability, especially the method invented by M. Ferranti. The author's own method is an improvement on this. Having made for various ships numerous drawings which showed on the vertical section of the ship, in length and in direction, the arms of the righting levers, for various draughts and inclinations, he conceived the idea of joining by continuous lines the extremities of these arms corresponding to the same angle of inclination. Taking such angles at intervals of 10°, he thus obtains a curve which he calls the "pantocarène isocline," and from these curves he obtains at once with complete accuracy and for all possible cases the usual curves of statical stability. The paper gives the principal properties of these curves, together with the mode of their calculation and various examples of working. The paper had been translated by Sir E. J. Reed, who may therefore be considered to have lent his authority to the value of the method proposed. The discussion on these papers turned mainly on unimportant and to some extent personal questions, and, though animated, does not need production here.

On Thursday evening, April 3, the most important paper was one by Mr. James Howden on Combustion of Fuel in Furnaces and Steam Boilers by Natural Draught and by Supply of Air under Pressure. The object of it was to describe a new boiler on which the author was experimenting, and which, if his account be correct, is likely to realise very important advantages in the way of economy of fuel. The experiments are not concluded, but the author considers them to justify him in claiming a most extraordinary economy as compared with ordinary marine boilers. Taking the instance of the Oregon, the latest-built of the swift Atlantic liners, he professes that the coal consumption might be reduced from 31,000 to 19,000 lbs. per hour, with an equal supply of steam and with a diminution in the fire-grate surface from 1512 to only 641 square feet. In the discussion which ensued very grave doubts were expressed as to the reality of such a saving, and it would probably have been more wise if the author had completed his experiments before claiming so very large a step in advance.

The next paper, by Mr. A. B. Broun, on the Application of Hydraulic Machinery to the Loading, &c., of Steamships, gave an interesting account of a complete hydraulic system applied to all the work required in an ordinary vessel, but did not raise any theoretical questions. A paper was then read by Mr. J. F. Hall, on Cast Steel as a Material for Crank Shafts. The author, who belongs to the well known firm of Messrs. Jessop of Sheffield, advocated the making of these important parts of a ship by the ordinary method of casting steel, without any subsequent hammering or working. His view is that such hammering can never reach the centre of a large mass of steel, such as an ingot; and that even if it did it would not completely weld up and remove the cavities which are not infrequently found in that region. In fact his view was that forging actually did harm by consolidating the outer layers and preventing them from contracting subsequently, as the hotter interior shrank in its cooling. By using ordinary methods of casting, and taking care to have a sufficient head or column of metal standing up above the casting itself, he considered that all fear of cavities within the latter was removed. Any unsoundness would be found only within the column, which would of course be cut off when the casting was cool.

The remaining papers, read on Friday, will not require any extended notice. That of Mr. P. Jenkins, on the Construction of Metacentric Diagrams, was a theoretical paper, dealing with the problem of stability, and chiefly devoted to establishing the following theorem:—"For any position of the centre of gravity the initial righting moment is either a maximum or a minimum when the water plane is so placed that the centre of curvature of the curve of flotation is at the same height in the vessel as the centre of gravity." Another contribution to the same problem, that of stability, was read by Mr. S. Benjamin, and described a model or apparatus enabling a shipowner to determine the position of the centre of gravity of his vessel for any loading before she is loaded, and also the alteration of its position due to any subsequent change in the loading. Yet another paper, by Dr. A. Amsler, described the application of the integrating apparatus which bears that name to such calculations as those of the curves already mentioned in Mr. Denny's paper. Mr. J. E. Spence described a form of diagram exhibiting in a simple shape all the data depending on the form of a ship which are required for determining her stability, and also a simple and direct method of graphic calculation for attaining these data. Mr. Thomas Phillips read a paper on the comparative safety of the particular class of vessels known as "well-decked" steamers. These were formerly treated with some suspicion by underwriters, but great improvements have lately been made, some of which were described in the paper, and with these the vessel appears to be even safer than what are called "flush" ships. Lastly, Mr. A. Taylor described a special instrument invented by him, and called a Stability Indicator, for determining the initial stability and stowage of ships at any displacement.

Mr. H. H. West read a paper on the Riveting of Iron Ships, giving tables for calculating the plate and rivet area for double-riveting, treble-riveting, and quadruple-riveting. He referred to the researches of Sir Edward Reed, the Institution of Mechanical Engineers, and others, but did not mention the modification of treble-riveting proposed some years ago, and lately carried into effect by a firm in Holland. On this system, in the middle row of the three rows of rivets, the rivets are spaced only half the distance apart of the two outer rows, the result being to increase very largely the proportion of strength. Capt. Heathorn described an arrangement called by him a Water-brake, for stopping the way of a ship in cases of collision or otherwise; and finally, Mr. J. E. Liardet described an apparatus for indicating the position of a ship's helm.

On the whole the Institution is to be congratulated on the interest and importance of the papers provided for it, and still more on the vigour and ability with which they were discussed by the very eminent engineers and shipbuilders who thronged the rooms of the Society of Arts for the purpose.

SCIENTIFIC SERIALS

Bulletin de l'Académie R. de Belgique, January 5.—On the existence of a fourth species (*B. borealis*) of the genus Balænoptera in the North Atlantic and Arctic Oceans, by M. Guldberg.—On the action of chlorine on combinations of sulphur, and on organic oxysulphurets, third communication, by M. W. Spring.—Researches on spermatogenesis in the Selacians (*Scyllium catulus, Sc. canicula, Raja clavata*), the salamander and mammals, by Prof. A. Swaen.—Essays on the political history of the last three centuries, by M. Van Praet.—Biographical notice of the painter Michael Van Cocxyen of Mechlin, by M. Castan.

Atti della R. Accademia dei Lincei, January 20.—Letter from King Humbert announcing an additional annual grant of 400*l.* for the promotion of biological studies, to be distributed in prizes in any way the Academy may think fit.—Some philological remarks on the 104th Psalm, by Guidi Ignazio.—Notice of an unpublished work of Prince Federico Cesi, entitled "De Laserpiteo et Laserpitii pluvia," in the library of the Botanic Institute at Padua, by Prof. A. Favaro.—Note on the antiquities discovered at Ventimiglia, Montefiascone, Naples, Pompeii, and other parts of Italy during the month of December 1883, by S. Fiorelli.

February 3 and 4.—Notice of some unpublished writings of Galileo Galilei in the National Library of Florence, by Prof. Favaro.—Report on Prof. Bellonci's work "On the Segmentation of the Egg of the Axolotl," by S. Trinchese.—Report on Dr. G. Frattini's work "On Some Propositions in the Theory of Substitutions," by S. Battaglini.—Report on Dr. L. Macchiati's work on the chemical nature of chlorophyll, by S. Cannizzaro.—Observations of the solar spots and faculæ made at the Observatory of the Collegio Romano during the year 1883, by Pietro Tacchini.—On the temperature corresponding to the Glacial period, third note, by Pietro Blaserna.—On the extraordinary crepuscular phenomena observed during the last few months, by Lorenzo Respighi.—Contributions to the study of the carbopyrolic acid **a**, by G. L. Ciamician and Paolo Silber.—Remarks on the Veronese Chelonian (*Protosphargis veronensis*) discovered in 1852 in the Upper Chalk near St. Anna di Alfaedo in Valpolicella, by Giovanni Capellini.—Geological observations on the islands of the Tuscan Archipelago, by B. Lotti.—Reports on the competition for the Royal Prizes for Physics, History, and Geography for the year 1882, by Signors Cantoni and Villari.—Reports on the Ministerial prizes for the Philosophical, Social, and Natural Sciences for the year 1883, by Signors Bonatelli and Trinchese.

February 17.—Obituary notices of the late Pietro Canal and Edoardo Laboulaye, Members of the Academy, by the President.—On the practice of burying human bones stripped of the flesh in Neolithic times, by Luigi Pigorini.—Note on the antiquities discovered at Felonica, Este, Imola, and in other parts of Italy during the month of January 1884.—Remarks on some codices in the Angelica Library connected with patristic theology, by Enrico Narducci.—Note on the parabolic orbit of the comet (*e*) discovered by Hartwig at Strasburg on August 24, 1879, by E. Millosevich.—On a remarkable disposition of the isogonic lines of terrestrial magnetism observed in the eastern districts of the Valley of the Po (two illustrations), by Ciro Chistoni.

Rivista Scientifico-Industriale, February 15 and 29.—Description of a new apparatus for the measurement of electro-motor forces (four illustrations), by E. Reynier.—Mathematical demonstration and value of the angle of least deviation described by a ray of light in its passage through a prism (one illustration), by Giuseppe Vanni.—Practical determination of the metallic resistance and chemical reaction of an electrolytic circuit, by Eugenio Marchese.—On the causes of the remarkable after-glows witnessed in Italy and elsewhere in 1883–84, by Prof. Carlo Marangoni. The author compares these phenomena with others of an analogous character observed in various parts of Europe in the year 1869. On several grounds he infers that the pink and red glows could not have been produced by moisture disseminated in the atmosphere in the solid, liquid, or gaseous state. He concludes that they are due to the presence of dust or minute particles of sand, which absorb the coloured rays in the central region of the solar spectrum while transmitting the extreme colours—that is, red and violet. The paper, which is to be continued, offers no suggestion as to the possible origin of the particles of dust to which the phenomena are attributed.—Note on the extinct and living mollusks of the Gardone district, by Prof. Strobel.—On the fossil insects of the Carboniferous schists of Commentry, by S. Brongniart.—Note on the limits of diatomaceous vegetation in marine basins, by Count A. F. Castracane.

Rendiconti del Reale Istituto Lombardo, February 21.—Biographical notice of Carlo Tenca and his times, by Prof. Giovanni Cantoni.—Some reflections on the results of the recent examinations in the Italian language and literature in the higher schools of the Peninsula, by Prof. C. Baravalle.—Fresh researches on the oxidation of sulphur, with some remarks on the oxidising power of the so-called atomic oxygen and of ozone, by Prof. E. Pollacci.—On some cases of subcutaneous nervous affections caused by the presence of Oscyuris, Tænia, Solium, and other parasites, by Prof. A. Scarenzio.—On the relations between the malady known as "bronze skin," and the changes in the suprarenal blood capsules, by Prof. G. Sangalli.—Meteorological observations made in the Brera Observatory, Milan, during the month of February 1884

SOCIETIES AND ACADEMIES

London

Royal Society, March 27.—"On the Electro-chemical Equivalent of Silver, and on the Absolute Electromotive Force of Clark Cells." By Lord Rayleigh, D.C.L., F.R.S.

The investigations upon this subject which have been carried on by Mrs. Sidgwick and myself during the last year and a half, though not yet quite finished, are so far advanced that no doubt remains as to the general character of the results; and as these results have application in the daily work of practical electricians, it is thought desirable to communicate them without further delay.

The currents are measured by balancing the attraction and repulsion of coaxal coils against known weights, as described before the British Association in 1882, a method which has fully answered the favourable expectations then expressed. To what was said on that occasion it will be sufficient for the present to add that the readings are taken by *reversal* of the current in the fixed coils, and the difference of weights thus found (about 1 grm.) represents the double force of attraction free from errors depending upon the connections of the suspended coil, and other sources of disturbance.

The difficulties which have been experienced, and which have been the cause of so much delay, have related entirely to the behaviour of the silver voltameters, of which never less than two, and sometimes as many as five, have been included in the circuit of the measured current. In order to render the deposit more compact, and thus to diminish the danger of loss in the subsequent manipulations, *acetate* of silver was added in the earlier experiments to the standard solution of nitrate. Experience, however, has shown that the principal risk is not in the loss of metal, but in the obstinate retention of salt within the fine pores of the deposit, leading to an over-estimate of the amount. When the texture is very compact, this danger increases, and deposits from a solution containing acetate are often decidedly too heavy, even after the most careful and protracted washings. On heating to low redness a portion, at any rate, of the retained salt is decomposed NO_2 is driven off, and a loss of

weight ensues. With pure nitrate, to which we finally recurred, the risk is much less.

The actual weights of deposited silver were usually from 2 to 3 grms., and, so far as the mere weighings are concerned, should have been correct to 1/10,000. Discrepancies three or four times as great as this are, however, actually met with, whether due to retention of salt or to loss of metal it is difficult to say. The final number, expressing in C.G.S. measure the electro-chemical equivalent of silver, is a little lower than that ($1\cdot119 \times 10^{-2}$) given on a previous occasion (*Cambridge Proceedings* for November 26, 1883). It approximates closely to $1\cdot118 \times 10^{-2}$, and is thus in precise agreement with the number announced within the last few weeks by Kohlrausch, viz. $1\cdot1183 \times 10^{-2}$. Its substantial correctness can therefore hardly be doubted, more especially as it does not differ very much from the number (1·124) obtained by Mascart. In terms of practical units, we may say that the ampere current deposits per hour 4·025 grms. of silver.

When we are provided with means for the absolute measurement of currents, the determination of electromotive force is a very simple matter if we assume a knowledge of absolute resistance. A galvanic cell is balanced against the known difference of potentials generated by a known current in traversing a known resistance. The difficulty relates entirely to the preparation and definition of the standard cells. A considerable number of Clark cells have been set up and tested at intervals during the last six months, and their behaviour has been satisfactory, the extreme range after the first ten days) not much exceeding 1/1000. A modified form of cell, in which the solid zinc is replaced by an amalgam, is at present under trial.

In Mr. Latimer Clark's own determination the B.A. unit is assumed to be correct, and the E.M.F. of the cell at 15° C. was found to be 1·457 volt. *On the same assumption* we obtain the not greatly differing value 1·453 volt. If we take the true value of the B.A. unit as ·9867 ohm, 1·453 will be replaced by 1·434.

Experiments are also in progress to determine in absolute measure the rotation of the plane of polarisation of light in bisulphide of carbon under the action of magnetic force. Of the results obtained by Gordon and Becquerel, differing by about 9 per cent., our preliminary measurements tend rather to confirm the former.

Mathematical Society, April 3.—Prof. Henrici, F.R.S., president, in the chair.—The Rev. A. C. E. Blomfield was admitted into the Society.—The following communications were made:—On double algebra, by Prof. Cayley, F.R.S.—On the homogeneous and other forms of equation of a plane section of a surface, by J. J. Walker, F.R.S.—A direct investigation of the complete primitive of the equation $F(x, y, z, p, q) = 0$, with a way of remembering the auxiliary system, by J. W. Russell.—On electrical oscillations and the effects produced by the motion of an electrified sphere, by J. J. Thomson.

Chemical Society, March 31.—Anniversary Meeting.—Dr. W. H. Perkin, F.R.S., president, in the chair.—The President read his annual address. The number of Fellows is at present 1324. During the past twelve months the Society has lost by death nineteen Fellows, including Sir C. W. Siemens, Messrs. W. Spottiswoode, J. T. Way, and J. Young. After briefly alluding to the more important advances in chemical science, the president drew attention to the fact that the number of original papers read before the Society had steadily decreased since 1881, notwithstanding the steady increase in the number of Fellows, and the greater facilities for the study of chemistry now offered by the numerous laboratories recently opened. The Longstaff Medal was awarded to Mr. O'Sullivan. The following Officers and Council were elected:—President: Dr. W. H. Perkin, Ph.D., F.R.S.; Vice-Presidents: Sir F. A. Abel, Warren De La Rue, E. Frankland, J. H. Gilbert, J. H. Gladstone, A. W. Hofmann, W. Odling, Sir Lyon Playfair, H. E. Roscoe, A. W. Williamson, P. Griess, G. D. Liveing, E. Schunck, T. E. Thorpe, A. Voelcker, W. Weldon; Secretaries: H. E. Armstrong, J. Millar Thomson; Foreign Secretary: H. Müller; Treasurer: W. J. Russell; Members of Council: E. Atkinson, H. T. Brown, T. Carnelly, M. Carteighe, R. J. Friswell, W. R. E. Hodgkinson, D. Howard, F. R. Japp, R. Meldola, R. Messel, C. O'Sullivan, C. Schorlemmer.

Geological Society, March 22.—Prof. T. G. Bonney, F.R.S., president, in the chair.—The Rev. Frank Ballard, M.A., was proposed as a Fellow of the Society.—The following communications were read:—On *Rhytidosteus capensis*, Owen, a Labyrinthodont Amphibian from the Trias of the Cape of Good Hope, by Sir Richard Owen, K.C.B., F.R.S. The author first noticed the discovery of certain forms of Amphibia belonging to the genera *Labyrinthodon*, *Brachyops*, *Petrophryne*, and *Rhinosaurus*, and called attention to certain typical peculiarities in the structure of the teeth, the form of the bony palate, and the double occipital condyle. An imperfect cranium of the species now described as *Rhytidosteus capensis* was procured by Mr. Heer in the Orange Free State from the Trias of Swanepol, Beersheba, and deposited by him in the Bloemfontein Museum. This specimen, which was brought to England and submitted to the author by Dr. Exton, consists of the anterior portion of the skull with part of the mandible attached. The general form is batrachoid, and one of the hinder palato-vomerine teeth, on being examined microscopically, exhibited the characteristic labyrinthodont structure. The surface of the skull, and the characters of the premaxillary, nasal, frontal, and prefrontal bones were described. The parietals and postfrontals are imperfect, the hinder part being lost. The rami of the mandible are also imperfect behind, but a broken fragment shows the articular surface. The vomerine bones were also described, with the posterior nostril and the teeth before and behind this opening. The breadth of the bony palate at its hinder fractured border is 5 inches; the length of the part preserved 4½ inches; the mandible, when perfect, was probably from 11 inches to a foot in length. The author also gave an account of the dentition wielded by the premaxillary, maxillary, vomerine, palatine, and mandibular bones. The author pointed out that the type of air-breathing vertebrates to which the present genus belongs reached its highest development in the Triassic period in Britain, Russia, North America, Hindostan, and South Africa. The only known antecedent form from which the labyrinthodont structure of tooth might have been derived is a genus of fishes named *Dendrodus*, in the Old Red Sandstone. The Liassic Ichthyosaurs also show some similarity in tooth-structure; but in them there is far greater simplicity.—On the occurrence of antelope-remains in Newer Pliocene beds in Britain, with the description of a new species, *Gazella anglica*, by E. Tulley Newton, F.G.S.—A comparative and critical revision of the Madreporaria of the White Lias of the Middle and Western Counties of England, and of those of the Conglomerate at the base of the South-Wales Lias, by Robert F. Tomes, F.G.S.

Zoological Society, April 1.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—Prof. Flower exhibited and made remarks on a series of skulls of the Bottle-nosed Whale (*Hyperoodon rostratus*), illustrating the various stages presented by this animal as regards the conformation of its skull in the different ages of both sexes. Prof. Flower also exhibited, on behalf of Messrs. Langton and Bicknell, a specimen of spermaceti obtained from the head of the *Hyperoodon*.—Mr. Sclater exhibited and made remarks on specimens of the eggs of two species of Testudinata (*Testudo elephantopus*, and *Chelys matamata*) recently laid by animals living in the Society's Gardens. —Mr. R. Bowdler Sharpe exhibited and made remarks on a Red-throated Pipit (*Anthus cervinus*) caught near Brighton in March last. Mr. Sharpe exhibited at the same time an example of the true Water-Pipit (*Anthus spinoletta*) captured at Lancing, in Sussex, in March 1877.—Prof. E. Ray Lankester, F.R.S., exhibited and made remarks on a large living Scorpion (*Buthus cyaneus*) from Ceylon.—A communication was read from Prof. T. Jeffrey Parker, being the first of a series of studies in New Zealand Ichthyology. The present paper gave a description of the skeleton of *Regalecus argenteus*. The species was founded on a specimen cast ashore at Moeraki, Otago, in June 1883.—A communication was read from Viscount Powerscourt, F.Z.S., containing an account of the origin and progress of the herd of Japanese Deer at Powerscourt.—A communication was read from Mr. G. A. Boulenger, giving the diagnoses of some new Reptiles and Batrachians from the Solomon Islands, collected and presented to the British Museum by Mr. H. B. Guppy, of H.M.S. *Lark*.—A communication was read from Mr. C. O. Waterhouse, containing an account of the coleopterous insects collected by Mr. H. O. Forbes in the Timor-Laut Islands.—Mr. F. D. Godman, F.R.S., read a paper containing an account of the Lepidoptera collected by the late Mr. W. A. Forbes on the banks of the Lower Niger, the Rhopalocera being described by Messrs. F. D. Godman and O. Salvin, and the Heterocera by Mr. H. Druce. The species of butterflies were fifty in number, and comprised representatives of all the families of Rhopalocera hitherto known from Tropical Africa, except the Erycinidæ, a

group but feebly developed in this region.—Mr. R. Bowdler Sharpe read the description of three rare species of Flycatchers, viz. *Alseonax minima*, *Lioptilus abyssinicus*, and *Lioptilus galinieri*. Mr. Sharpe also described an apparently new species of Nuthatch discovered by Mr. John Whitehead in the mountains of Corsica, and proposed to be called *Sitta whiteheadi*.—Mr. G. E. Dobson, F.R.S., read a paper on the myology and visceral anatomy of *Capromys melanurus*, of which rare mammal specimens had been lately obtained for him by Mr. F. W. Ramsden, H.M.'s Consul at St. Jago de Cuba. The well-known division of the hepatic lobes into minute lobules in *C. pilorides* from the same island was shown not to exist in *C. melanurus*, which otherwise closely resembled the former species, and this character could therefore no longer be considered a generic one.

EDINBURGH

Royal Society, March 3.—Sir W. Thomson, hon. vice-president, in the chair.—Sir W. Thomson communicated a paper on the efficiency of clothing for maintaining temperature. He showed that if a body be below a certain size, the effect of clothing will be to cool it. In a globular body the temperature will only be kept up if the radius be greater than $\frac{k}{2e}$, where k is the conductivity of the substance and e its emissivity.—Prof. J. Thomson read a paper on the law of inertia, the principle of chronometry, and the principle of absolute clinural rest and of absolute rotation. In this paper the author proceeded to discriminate between what men can know, and what men cannot know, as to rest and motion in unmarked space. For example, men have no means of knowing or imagining whether a ball existing in space is in motion or at rest; nor have they any means, if it be in motion, of knowing or imagining any one direction, rather than another, as being the direction of the straight line from the place that was occupied by its centre at any past instant to the place occupied by that centre at present. There is then an essential difficulty as to our forming a distinct conception either of rest or of rectilinear motion through unmarked space. He discussed, in connection with this, the statement set forth by Sir Isaac Newton, under the designation of the first law of motion, that *every body continues in its state of resting or of moving uniformly in a straight line, except in so much as, by applied forces, it is compelled to change that state*. A most important truth in the nature of things, perceived with more or less clearness, was, he said, at the root of that enunciation; but the words, whether taken by themselves, or in connection with Newton's accompanying definitions and illustrations, were inadequate to give expression to that great natural truth. He proceeded to explain the character of mutual motions, which can in any sense be regarded as uniform rectilinear mutual motions. He gave, under the title of the law of inertia, an enunciation which he offered as setting forth, by a better expression, all the truth which is either explicitly stated, or is suggested by the first and second laws of motion in Sir Isaac Newton's arrangement. In connection with the law of inertia he gave further statements bringing out expressions of the principle of chronometry and the principle of "directional fixedness" or of absolute clinural rest, and of absolute rotation.—Sir W. Thomson described a modification of Gauss's method for determining the horizontal component of terrestrial magnetic force and the magnetic moments of bar magnets in absolute measure.—Mr. Thomas Muir gave a paper on the phenomenon of greatest middle in the cycle of a class of periodic continued fractions.

March 17.—Robert Grey, vice-president, in the chair.—Messrs. Peach and Horne, of the Geological Survey of Scotland, communicated a paper on the Old Red Sandstone volcanic rocks of Shetland.—Mr. P. Geddes gave the first two parts, mathematical and physical, of a paper on the principles of economics.—Prof. Crum Brown communicated a paper by Prof. Michie Smith on an integrating hygrometer.

DUBLIN

University Experimental Science Association, March 18.—On the boiling-points of the haloid ethers, by F. Trouton.—On a new test for gallic acid, by A. E. Dixon, B.A. The crimson-red colour which Dr. Sidney Young had noticed on adding a solution of cyanide of potassium to a solution of gallic acid, and which a few minutes' rest or gentle warmth causes completely to disappear, is probably due to oxidation. For although when shaken in contact with the air the colour reappears, it will not do so when shaken in an atmosphere of hydrogen, nitrogen, or carbon dioxide. On re-exposure to the air, with agitation, the colour may be brought back. The red colour is not dissolved out by alcohol, ether, or chloroform; neither does it afford any characteristic absorption-spectrum.—On Ayrton and Perry's electrometers, by G. F. Fitzgerald, F.R.S.—An electro-magnet for use in analysis was exhibited by J. Joly, B.E. The electro-magnet is sealed into a test-tube to enable it to be dipped into solutions containing perruginous particles.

PARIS

Academy of Sciences, March 31.—M. Rolland in the chair.—Remarks on the third volume of the "Annals of the Bureau of Longitudes," presented to the Academy by M. Faye.—On a proposed classification of comets according to their direct or retrograde motion, by M. Faye.—Note on the form of the nucleus of the Pons-Brooks comet, by M. Faye.—On the specific heat of gaseous elements at very high temperatures, by MM. Berthelot and Vieille.—Note on the origin of sugar of milk, by M. Paul Bert. From experiments made on goats the author infers that the sugar of milk is produced by the mammary secretion of the superabundant sugar formed by the organisms after parturition, most probably in the liver.—On a new species of fossil Sirenian found in the Paris Basin, by M. A. Gaudry.—On the correspondence between two different species of functions of two systems of quantities correlated and equal in number, by M. Sylvester.—Separation of gallium; separation from organic substances, by M. Lecoq de Boisbaudran.—On a modified form of lightning-conductor, by M. A. Callaud.—Results of experiments with a new ventilating system worked by centrifugal force, by M. L. Ser.—Observations made at the Meudon Observatory on the planet Mars, by M. E. L. Trouvelot.—Approximate calculation of the thrust and surface of fracture in a homogeneous horizontal mass of earth supported by a vertical wall, by M. J. Boussinesq.—On Gyldén's differential equation:—

$$\frac{d^2 x}{d t^2} = \phi_0 + x\phi_1 + x^2\phi_2 + \ldots + x^m\phi_m + \ldots$$

in which the ϕ's are trigonometrical series, by M. Poincaré.—Distribution of the potential in a rectangular plate traversed by an electric current with permanent *régime*, by M. A. Chervet.—On the electric phenomenon of the transport of ions and its relation to the conductivity of saline solutions, by M. E. Bouty.—On the resistance of the carbons employed in the electric light of the French lighthouses, by M. F. Lucas.—Note on the verification of the laws of transverse vibration in elastic rods, by M. E. Mercadier.—The general theory of dissociation deduced from the general data furnished by the mechanical theory of heat, by M. Isambert.—Note on the measurement of the tension of dissociation in the iodide of mercury, by M. L. Troost.—On the phenomenon of the crystalline superheating of sulphur, by M. D. Gernez.—On the non-existence of the hydrate of ammonium, by M. D. Tommasi. The author's experiments lead him to the conclusion already arrived at by Thomsen, that hydrate of ammonium does not exist in ammoniac water.—On the decomposition by water of the combinations of cupreous chloride with the chloride of potassium and chlorhydric acid, by M. H. Le Chatelier.—On the composition of pitch-blende, by M. Blomstrand. From his analysis the author concludes that this substance is a mixture of uranine, silicates, carbonate of lime, and sulphuret of iron, its formula being:—

$$2U_7Pb(O_6U_3) + 5(R_2O_4Si, 2Aq) + 7(CaO_2CO) + \tfrac{1}{8}\,Fe_7S_8.$$

—Note on the quantitative analysis of the phosphoric acid in arable lands, by M. G. Lechartier.—Heat of formation of the fluoride of silver, of magnesium, and of lead, by M. Guntz.—Thermochemical study of hydrofluosiliceous acid, by M. Ch. Truchot.—On the glyoxalbisulphide of soda, by M. de Forcrand.—On the influence of cerebral lesions on the temperature of the body, by M. Ch. Richet.—On the special distribution of the motor roots of the brachial plexus in the human system, by MM. Forgue and Lannegrace.—Description of a gigantic Dictyoneura (*D. monyi*) found in the Carboniferous measures of Commentry (Allier), by M. Ch. Brongniart. This remarkable insect must have been at least fifty centimetres long.—On the origin of the roots in the ferns, by M. Lachmann.—On the causes which may modify the effects of the action of light in directing the motion of plants, by M. E. Mer.—On the diffusion of christianite in the ancient lavas of the Puy-de-Dôme and the Loire Basin, by M. F. Gonnard.—Note on the origin of certain phosphates of lime found in mass in the limestones of the Secondary series, and of

certain iron ores belonging to the class of globular ores, by M. Dieulafait.—On the solar halos observed at Saint Maur on the morning of March 29, by M. E. Renou.—Note on the presence of manganese in the wines of Grave, by M. E.-J. Maumené.

BERLIN

Physical Society, March 7.—Prof. Neesen, by means of different glass tubes, demonstrated certain phenomena of Kundt's dust figures produced by experimenting with deep tones. Busied with an examination into the cause, not yet explained, of the transverse ridgings in sounding-tubes, Prof. Neesen has, instead of the high tones of longitudinally-vibrating tubes, tested deeper tones, which are kept up in the column of air of the glass tubes by an electric tuning-fork. In the course of this investigation he made very beautiful observations in many tubes of dust-whirls roaming hither and thither, now to one side, now to the other. In other tubes, again, these whirls came to light either with great difficulty or but imperfectly. It would therefore appear that the material of the tubular wall exercised some influence on the production of those whirls. The speaker had yet, however, come to no definite result respecting the cause of the transverse ridges.—Dr. Kœnig supplemented the experiments he communicated at the last sitting of the Society, on the sensitiveness of normal eyes for variations of colour between the wave-lengths of 640 and 430. This he had so far done, inasmuch as he had tested the influence of light-intensity on the sensibility in question. Seeing, as was well known, that light-intensity, in this part of the spectrum especially, mounted very rapidly from the line C to the line D, and again sank from the maximum beyond D down to F, it would be possible that the differentiating sensibility arrived at in the former experiments was in large part conditioned by the differences of intensity. The cooperation of intensity was now in the new experiments partly excluded as a factor in this way, that the spectrum was observed through an absorbing medium whose maximum of absorption stood at D, so that the curve of light-intensity between C and D rose with much less rapidity, ran horizontally for some distance, and then sank to D. The measurements, being carried out as in the former experiments, yielded the result that the differentiative sensibility under the conditions mentioned had undergone very little alteration, and that, consequently, light-intensity had no influence on the range that had been arrived at.—Prof. von Helmholtz reported on a theoretic treatise he had laid before the Berlin Royal Academy, in which he had taken in hand the task of explaining, in accordance with mechanical principles, thermal movements, and more particularly Carnot's law. He attained his object by means of the rules bearing on stationary movements, as they were calculated for a vortex revolving without friction and with great velocity, or for a fluid moving without friction in a closed circular canal. The equations for these stationary movements derived from mechanics corresponded with those derived from Lagrange's law for thermal movements.

Physiological Society, March 14.—Prof. Lucae gave an address on the subject of subjective auricular sensations and their treatment. He showed by examples that the idea that subjective auricular sensations, and in particular the generally known one of singing in the ears, had a somatic cause, such as stoppage of the external acoustic duct or of the Eustachian tube, was not in accordance with experience. Both on himself and on persons of musical culture he had determined the pitch of the singing or whistling sound, and had found it equal to the proper tone of the external acoustic duct. This circumstance, together with several other facts, led him to the conjecture that the singing in the ears was caused by a tetanus of the tensor tympani, which set the air over the membrane of the tympanum in continuous oscillation. In cases of suffering from this distemper, of which the speaker cited a number of examples, the subjective auricular sensations were to be divided into such as were intensified and such as were abated by external sounds. Both kinds were to be regarded as phenomena of abnormal resonance, and were accompanied by different degrees of hardness of hearing down to deafness. The treatment of these subjective sensations, so far as they were simple tones and noises, and not the subjective hearing of words or of anything outside the hearer (disturbances psychological and beyond the scope of his address) consisted, in the opinion of the speaker, an opinion based on manifold personal experience, in subjecting the sufferers, for progressively longer periods of time, and for as many as two to three minutes at once, to a certain constant tone of the tuning-fork. In such a case Prof. Lucae used deep tuning-forks with such as heard subjective high tones, and *vice versâ*. With the cessation of the subjective noises the deafness also usually disappeared, and the sufferers recovered a permanently normal state in this respect. An explanation of this phenomenon the speaker thought might be found in the analogy of other sensations in which abnormal excitement in one part of the sensory nerves was relieved by the excitement of neighbouring nervous parts.—Prof. Munk reported on a treatise sent for insertion in the *Verhandlungen* by Dr. Gad, a foreign member. Contrary to the opinion on the subject hitherto entertained, Dr. Gad in this treatise proved that in the spinal marrow of frogs, even under the seventh nerve-root, there were reflex centres in operation. By cutting through the spinal marrow below this spot, reflex convulsions from the toes upwards are produced, not only on the same but also on the opposite side. In other experiments on frogs the spinal marrow was cut through beneath the medulla oblongata, and the upper part of the spinal marrow as far as the second vertebra carefully prepared and laid on filtering paper saturated with strychnine. On stimulating the frog at the lower extremities reflex movements were seen to pervade the whole body, but in the region of those sections of the spinal marrow treated with strychnine, flexor spasms were observed, though it is well known to be a special characteristic of the strychnine spasm that it exclusively attacks the extensor muscles. In this way was demonstrated the existence of conducting tracks rising from the reflex centres situated in the lowermost part of the spinal marrow up to its topmost parts. If these latter, again, were electrically stimulated, no flexor movement could be started from the spot which before, under the operation of strychnine, had generated exterior reflections. Between this part of the spinal marrow and the motory nerves there must therefore lie ganglia.—Following up his communication at the last sitting, on the presence of nitric acid in urine, Dr. Weyl brought before the Society a series of chemical reactions tending to demonstrate that nitric acid could exist and be substantiated in an oxidised solution along with urea.

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